

at low temperatures. Without them, the crowning achievement of obtaining hydrogen in the liquid state (May, 1898) would scarcely have been possible. Prof. Dewar is shown handling one of these vessels in the picture on p. 461.

The researches carried out under the transcendental conditions now available at the Royal Institution have led to many surprises: notably is this true of the investigations carried out by Profs. Dewar and Fleming on the electrical conductivity of metals, and on specific inductive capacity. The fact that almost all substances may be rendered phosphorescent by insolation when cooled to low temperatures is another discovery made by Prof. Dewar which promises to be of special importance in the light of recent researches on radio-activity.

But to understand Prof. Dewar fully, it is necessary to know him in the upper as well as in the lower regions of the Royal Institution; not only the wealth of his powers of imagination and his scientific acumen then become apparent, but it is realised that he is a man of extraordinarily sympathetic nature, penetrated with artistic feeling and emotions. Unfortunately, he is also gifted with a reticence rare among artists, which is particularly manifest when the time comes to commit his thoughts to paper; the world has lost much in not being made fully acquainted with his discoveries, and if his reflections were more frequently uttered outside his private circle, it would be to the advantage of scientific progress. We may hope that there is much time left to him in which to repair minor faults such as these.

A laboratory in which so many remarkable and important discoveries have been made may certainly be said to have justified the hopes of its founder, and it is surprising that its successes have not won for it a larger measure of public support—that as yet it has had no imitators.

But there is one respect in which Count Rumford might well deplore failure. However much the lectures delivered in the Institution may have interested and even amused the rich, they have failed to lead them to appreciate in any proper measure the value of scientific research to the nation, a subject on which Davy dwelt much in his lectures; for had they done so, an industry such as the coal-tar colour industry, so closely connected in its origin with the Institution, which was first established and for a time flourished in this country, would not have been allowed to pass almost entirely into other hands; the attempt made by Davy to raise agriculture to a science would have been persevered in at the public cost; electrochemistry would have been steadily developed; and pioneer work such as Faraday did on iron and glass would not have been allowed to stand in splendid isolation. A century of the highest example has had little effect in making the knowledge of scientific method a public possession.

#### THE BELFAST MEETING OF THE BRITISH ASSOCIATION.

IN previous issues of NATURE, particulars have been given as to the local arrangements which have been made for the comfort of those attending this meeting, and the titles of the papers which may be expected to be read in the various sections have been published; not much remains, therefore, to be said by us on this occasion. It may, however, be stated that the illustrated handbook or guide issued by the Association and prepared under the auspices of the Belfast Naturalists' Field Club appears to have been very carefully compiled. It deals with the subjects respectively of Belfast, geology, botany, zoology and antiquities, and is the work of many writers. So far as can be seen as we go to press, the meeting will be a successful one, it being estimated that in point of numbers attending it

will equal the gathering of 1874, at which the total attendance was 1951. Given fine weather, the meeting should be no less enjoyable and interesting than many of its predecessors. It had been hoped that the *Scotia*, of the Scottish Antarctic Expedition, would have been able to visit the harbour and be open for inspection by the members of the Association; this hope, however, seems likely to be disappointed. The address of the President, Prof. Dewar, was delivered as we went to press yesterday, and the various sections began their proceedings this morning. In this issue we print the Presidential Address and that of the President of Section A. Other addresses and accounts of the papers and reports brought before the sections will duly appear in subsequent numbers.

INAUGURAL ADDRESS BY PROF. JAMES DEWAR, M.A., LL.D., D.Sc., F.R.S., PRESIDENT OF THE ASSOCIATION.

THE members of an Association whose studies involve perpetual contemplation of settled law and ordered evolution, whose objects are to seek patiently for the truth of things and to extend the dominion of man over the forces of nature, are even more deeply pledged than other men to loyalty to the Crown and the Constitution which procure for them the essential conditions of calm security and social stability. I am confident that I express the sentiments of all now before me when I say that to our loyal respect for his high office we add a warmer feeling of loyalty and attachment to the person of our Gracious Sovereign. It is the peculiar felicity of the British Association that, since its foundation seventy-one years ago, it has always been easy and natural to cherish both these sentiments, which indeed can never be dissociated without peril. At this, our second meeting held under the present reign, these sentiments are realised all the more vividly, because, in common with the whole empire, we have recently passed through a period of acute apprehension, followed by the uplifting of a national deliverance. The splendid and imposing coronation ceremony which took place just a month ago was rendered doubly impressive both for the King and his people by the universal consciousness that it was also a service of thanksgiving for escape from imminent peril. In offering to His Majesty our most hearty congratulations upon his singularly rapid recovery from a dangerous illness, we rejoice to think that the nation has received gratifying evidence of the vigour of his constitution, and may, with confidence more assured than before, pray that he may have length of happy and prosperous days. No one in his wide dominions is more competent than the King to realise how much he owes, not only to the skill of his surgeons, but also to the equipment which has been placed in their hands as the combined result of scientific investigation in many and diverse directions. He has already displayed a profound and sagacious interest in the discovery of methods for dealing with some of the most intractable maladies that still baffle scientific penetration; nor can we doubt that this interest extends to other forms of scientific investigation, more directly connected with the amelioration of the lot of the healthy than with the relief of the sick. Heredity imposes obligations and also confers aptitude for their discharge. If His Majesty's royal mother throughout her long and beneficent reign set him a splendid example of devotion to the burdensome labours of State which must necessarily absorb the chief part of his energies, his father no less clearly indicated the great part he may play in the encouragement of science. Intelligent appreciation of scientific work and needs is not less but more necessary in the highest quarters to-day than it was forty-three years ago, when His Royal Highness the Prince Consort brought the matter before this Association in the following memorable passage in his Presidential Address: "We may be justified, however, in hoping that by the gradual diffusion of science and its increasing recognition as a principal part of our national education, the public in general, no less than the legislature and the State, will more and more recognise the claims of science to their attention; so that it may no longer require the begging box, but speak to the State like a favoured child to its parent, sure of its paternal solicitude for its welfare; that the State will recognise in science one of its elements of strength and prosperity, to protect which the clearest dictates of self-interest demand." Had this advice been seriously taken to heart and acted upon by the rulers of the nation at the time, what splendid

results would have accrued to this country! We should not now be painfully groping in the dark after a system of national education. We should not be wasting money, and time more valuable than money, in building imitations of foreign educational superstructures before having put in solid foundations. We should not be hurriedly and distractedly casting about for a system of tactics after confrontation with the disciplined and co-ordinated forces of industry and science led and directed by the rulers of powerful States. Forty-three years ago we should have started fair had the Prince Consort's views prevailed. As it is, we have lost ground which it will tax even this nation's splendid reserves of individual initiative to recover. Although in this country the king rules, but does not govern, the Constitution and the structure of English society assure to him a very potent and far-reaching influence upon those who do govern. It is hardly possible to overrate the benefits that may accrue from his intelligent and continuous interest in the great problem of transforming his people into a scientifically educated nation. From this point of view we may congratulate ourselves that the heir to the Crown, following his family traditions, has already deduced from his own observations in different parts of the empire some very sound and valuable conclusions as to the national needs at the present day.

*Griffith—Gilbert—Cornu.*

The saddest yet the most sacred duty falling to us on such an occasion as the present is to pay our tribute to the memory of old comrades and fellow-workers whom we shall meet no more. We miss to-day a figure that has been familiar, conspicuous, and always congenial at the meetings of the British Association during the last forty years. Throughout the greater part of that period Mr. George Griffith discharged the onerous and often delicate duties of the assistant general secretary, not only with conscientious thoroughness and great ability, but also with urbanity, tact and courtesy that endeared him to all. His years sat lightly upon him, and his undiminished alertness and vigour caused his sudden death to come upon us all with a shock of surprise as well as of pain and grief. The British Association owes him a debt of gratitude which must be so fully realised by every regular attendee of our meetings that no poor words of mine are needed to quicken your sense of loss, or to add to the poignancy of your regret.

The British Association has to deplore the loss from among us of Sir Joseph Gilbert, a veteran who continued to the end of a long life to pursue his important and beneficent researches with untiring energy. The length of his services in the cause of science cannot be better indicated than by recalling the fact that he was one of the six past Presidents boasting fifty years' membership whose jubilee was celebrated by the Chemical Society in 1898. He was in fact an active member of that Society for over sixty years. Early in his career he devoted himself to a most important but at that time little cultivated field of research. He strove with conspicuous success to place the oldest of industries on a scientific basis, and to submit the complex conditions of agriculture to a systematic analysis. He studied the physiology of plant life in the open air, not with the object of penetrating the secrets of structure, but with the more directly utilitarian aim of establishing the conditions of successful and profitable cultivation. By a long series of experiments, alike well conceived and laboriously carried out, he determined the effects of variation in soil, and its chemical treatment—in short, in all the unknown factors with which the farmer previously had to deal according to empirical and local rules, roughly deduced from undigested experience by uncritical and rudimentary processes of inference. Gilbert had the faith, the insight and the courage to devote his life to an investigation so difficult, so unpromising, and so unlikely to bring the rich rewards attainable by equal diligence in other directions, as to offer no attraction to the majority of men. The tabulated results of the Rothamsted experiments remain as a benefaction to mankind and a monument of indomitable and disinterested perseverance.

It is impossible for me in this place to offer more than the barest indication of the great place in contemporary science that has been vacated by the lamented death of Prof. Alfred Cornu, who so worthily upheld the best traditions of scientific France. He was gifted in a high degree with the intellectual lucidity, the mastery of form and the perspicuous method which characterise the best exponents of French thought in all departments of study. After a brilliant career as a student, he was chosen at

the early age of twenty-six to fill one of the enviable positions more numerous in Paris than in London, the Professorship of Physics at the Ecole Polytechnique. In that post, which he occupied to the end of his life, he found what is probably the ideal combination for a man of science—leisure and material equipment for original research, together with that close and stimulating contact with practical affairs afforded by his duties as teacher in a great school, almost ranking as a department of State. Cornu was admirable alike in the use he made of his opportunities and in his manner of discharging his duties. He was at once a great investigator and a great teacher. I shall not even attempt a summary, which at the best must be very imperfect, of his brilliant achievements in optics, the study of his predilection, in electricity, in acoustics and in the field of physics generally. As a proof of the great estimation in which he was held, it is sufficient to remind you that he had filled the highest presidential offices in French scientific societies, and that he was a foreign member of our Royal Society and a recipient of its Rumford medal. In this country he had many friends, attracted no less by his personal and social qualities than by his commanding abilities. Some of those here present may remember his appearance a few years ago at the Royal Institution, and more recently his delivery of the Rede Lecture at Cambridge, when the University conferred upon him the honorary degree of Doctor of Science. His death has inflicted a heavy blow upon our generation, upon France and upon the world.

#### *The Progress of Belfast.*

A great man has observed that the "intelligent anticipation of events before they occur" is a factor of some importance in human affairs. One may suppose that intelligent anticipation had something to do with the choice of Belfast as the meeting-place of the British Association this year. Or, if it had not, then it must be admitted that circumstances have conspired, as they occasionally do, to render the actual selection peculiarly felicitous. Belfast has perennial claims, of a kind that cannot easily be surpassed, to be the scene of a great scientific gathering—claims founded upon its scientific traditions and upon the conspicuous energy and success with which its citizens have prosecuted in various directions the application of science to the purposes of life. It is but the other day that the whole nation deplored at the grave of Lord Dufferin the loss of one of the most distinguished and most versatile public servants of the age. That great statesman and near neighbour of Belfast was a typical expression of the qualities and the spirit which have made Belfast what it is, and have enabled Ireland, in spite of all drawbacks, to play a great part in the Empire. I look round on your thriving and progressive city giving evidence of an enormous aggregate of industrial efforts intelligently organised and directed for the building up of a sound social fabric. I find that your great industries are interlinked and interwoven with the whole economic framework of the Empire, and that you are silently and irresistibly compelled to harmonious cooperation by practical considerations acting upon the whole community. It is here that I look for the real Ireland, the Ireland of the future. We cannot trace with precision the laws that govern the appearance of eminent men, but we may at least learn from history that they do not spring from every soil. They do not appear among decadent races or in ages of retrogression. They are the fine flower of the practical intellect of the nation working studiously and patiently in accordance with the great laws of conduct. In the manifold activities of Belfast we have a splendid manifestation of individual energy working necessarily, even if not altogether consciously, for the national good. In great Irishmen like Lord Dufferin and Lord Roberts, giving their best energies for the defence of the nation by diplomacy or by war, we have complementary evidence enough to reassure the most timid concerning the real direction of Irish energies and the vital nature of Irish solidarity with the rest of the Empire.

Belfast has played a prominent part in a transaction of a somewhat special and significant kind, which has proved not a little confusing and startling to the easy-going public. The significance of the shipping combination lies in the light it throws on the conditions and tendencies which make such things possible, if not even inevitable. It is an event forcibly illustrating the declaration of His Royal Highness the Prince of Wales, that the nation must "wake up" if it hopes to face its growing responsibilities. Belfast may plead with some justice that it, at least, has never gone to sleep. In various directions an immense advance has been effected during the



twenty-eight years that have elapsed since the last visit of the British Association. Belfast has become first a city and then a county, and now ranks as one of the eight largest cities in the United Kingdom. Its municipal area has been considerably extended, and its population has increased by something like 75 per cent. It has not only been extended, but improved and beautified in a manner which very few places can match, and which probably none can surpass. Fine new thoroughfares, adorned with admirable public institutions, have been run through areas once covered with crowded and squalid buildings. Compared with the early fifties, when iron shipbuilding was begun on a very modest scale, the customs collected at the port have increased tenfold. Since the introduction of the power-loom, about 1850, Belfast has distanced all rivals in the linen industry, which continues to flourish notwithstanding the fact that most of the raw material is now imported, instead of being produced, as in former times, in Ulster. Extensive improvements have been carried out in the port at a cost of several millions, and have been fully justified by a very great expansion of trade. These few bare facts suffice to indicate broadly the immense strides taken by Belfast in the last two decades. For an Association that exists for the advancement of science it is stimulating and encouraging to find itself in the midst of a vigorous community, successfully applying knowledge to the ultimate purpose of all human effort, the amelioration of the common lot by an ever-increasing mastery of the powers and resources of Nature.

#### *Tyndall and Evolution.*

The Presidential Address delivered by Tyndall in this city twenty-eight years ago will always rank as an epoch-making deliverance. Of all the men of the time, Tyndall was one of the best equipped for the presentation of a vast and complicated scientific subject to the mass of his fellow-men. Gifted with the powers of a many-sided original investigator, he had at the same time devoted much of his time to an earnest study of philosophy, and his literary and oratorical powers, coupled with a fine poetic instinct, were qualifications which placed him in the front rank of the scientific representatives of the later Victorian epoch, and constituted him an exceptionally endowed exponent of scientific thought. In the Belfast discourse Tyndall dealt with the changing aspects of the long unsettled horizon of human thought, at last illuminated by the sunrise of the doctrine of evolution. The consummate art with which he marshalled his scientific forces for the purpose of effecting conviction of the general truth of the doctrine has rarely been surpassed. The courage, the lucidity, the grasp of principles, the moral enthusiasm with which he treated his great theme have powerfully aided in effecting a great intellectual conquest, and the victory assuredly ought to engender no regrets.

Tyndall's views as a strenuous supporter and believer in the theory of evolution were naturally essentially optimistic. He had no sympathy with the lugubrious pessimistic philosophy whose disciples are for ever intent on administering rebuke to scientific workers by reminding them that, however much knowledge man may have acquired, it is as nothing compared with the immensity of his ignorance. That truth is indeed never adequately realised except by the man of science, to whom it is brought home by repeated experience of the fact that his most promising excursions into the unknown are invariably terminated by barriers which, for the time at least, are insurmountable. He who has never made such excursions with patient labour may indeed prattle about the vastness of the unknown, but he does so without real sincerity or intimate conviction. His tacit, if not his avowed, contention is, that since we can never know all it is not worth while to seek to know more; and that in the profundity of his ignorance he has the right to people the unexplored spaces with the phantoms of his vain imagining. The man of science, on the contrary, finds in the extent of his ignorance a perpetual incentive to further exertion, and in the mysteries that surround him a continual invitation, nay, more, an inexorable mandate. Tyndall's writings abundantly prove that he had faced the great problems of man's existence with that calm intellectual courage the lack of which goes very far to explain the nervous dogmatism of nescience. Just because he had done this, because he had, as it were, mapped out the boundaries between what is knowable though not yet known and what must remain for ever unknowable to man, he did not hesitate to place implicit reliance on the progress of which man is capable, through the exercise of patient and persistent research.

In Tyndall's scheme of thought the chief dicta were the strict division of the world of knowledge from that of emotion, and the lifting of life by throwing overboard the malign residuum of dogmatism, fanaticism, and intolerance, thereby stimulating and nourishing a plastic vigour of intellect. His cry was "Comotion before stagnation, the leap of the torrent before the stillness of the swamp."

His successors have no longer any need to repeat those significant words, "We claim and we shall wrest from theology the entire domain of cosmological theory." The claim has been practically, though often unconsciously, conceded. Tyndall's dictum, "Every system must be plastic to the extent that the growth of knowledge demands," struck a note that was too often absent from the heated discussions of days that now seem so strangely remote. His honourable admission that, after all that had been achieved by the developmental theory, "the whole process of evolution is the manifestation of a power absolutely inscrutable to the intellect of man," shows how willingly he acknowledged the necessary limits of scientific inquiry. This reservation did not prevent him from expressing the conviction forced upon him by the pressure of intellectual necessity, after exhaustive consideration of the known relations of living things, that matter in itself must be regarded as containing the promise and potency of all terrestrial life. Bacon in his day said very much the same thing: "He that will know the properties and proceedings of matter should comprehend in his understanding the sum of all things, which have been, which are, and which shall be, although no knowledge can extend so far as to singular and individual beings." Tyndall's conclusion was at the time thought to be based on a too insecure projection into the unknown, and some even regarded such an expansion of the crude properties of matter as totally unwarranted. Yet Tyndall was certainly no materialist in the ordinary acceptance of the term. It is true his arguments, like all arguments, were capable of being distorted, especially when taken out of their context, and the address became in this way an easy prey for hostile criticism. The glowing rhetoric that gave charm to his discourse and the poetic similes that clothed the dry bones of his close-woven logic were attacked by a veritable broadside of critical artillery. At the present day these would be considered as only appropriate artistic embellishments, so great is the unconscious change wrought in our surroundings. It must be remembered that, while Tyndall discussed the evolutionary problem from many points of view, he took up the position of a practical disciple of Nature dealing with the known experimental and observational realities of physical inquiry. Thus he accepted as fundamental concepts the atomic theory, together with the capacity of the atom to be the vehicle or repository of energy, and the grand generalisation of the conservation of energy. Without the former, Tyndall doubted whether it would be possible to frame a theory of the material universe; and as to the latter he recognised its radical significance in that the ultimate philosophical issues therein involved were as yet but dimly seen. That such generalisations are provisionally accepted does not mean that science is not alive to the possibility that what may now be regarded as fundamental may in future be superseded or absorbed by a wider generalisation. It is only the poverty of language and the necessity for compendious expression that oblige the man of science to resort to metaphor and to speak of the Laws of Nature. In reality, he does not pretend to formulate any laws for Nature, since to do so would be to assume a knowledge of the inscrutable cause from which alone such laws could emanate. When he speaks of a "law of Nature" he simply indicates a sequence of events which, so far as his experience goes, is invariable, and which therefore enables him to predict, to a certain extent, what will happen in given circumstances. But, however seemingly bold may be the speculation in which he permits himself to indulge, he does not claim for his best hypothesis more than provisional validity. He does not forget that to-morrow may bring a new experience compelling him to recast the hypothesis of to-day. This plasticity of scientific thought, depending upon reverent recognition of the vastness of the unknown, is oddly made a matter of reproach by the very people who harp upon the limitations of human knowledge. Yet the essential condition of progress is that we should generalise to the best of our ability from the experience at command, treat our theory as provisionally true, endeavour to the best of our power to reconcile with it all the new facts we discover, and abandon or modify it when it ceases to afford a coherent explanation of new experience. That procedure is far

as are the poles asunder from the presumptuous attempt to travel beyond the study of secondary causes. Any discussion as to whether matter or energy was the true reality would have appeared to Tyndall as a futile metaphysical disputation which, being completely dissociated from verified experience, would lead to nothing. No explanation was attempted by him of the origin of the bodies we call elements, nor how some of such bodies came to be compounded into complex groupings and built up into special structures with which, so far as we know, the phenomena characteristic of life are invariably associated. The evolutionary doctrine leads us to the conclusion that life, such as we know it, has only been possible during a short period of the world's history, and seems equally destined to disappear in the remote future; but it postulates the existence of a material universe endowed with an infinity of powers and properties, the origin of which it does not pretend to account for. The enigma at both ends of the scale Tyndall admitted, and the futility of attempting to answer such questions he fully recognised. Nevertheless, Tyndall did not mean that the man of science should be debarred from speculating as to the possible nature of the simplest forms of matter or the mode in which life may have originated on this planet. Lord Kelvin, in his Presidential Address, put the position admirably when he said "Science is bound by the everlasting law of honour to face fearlessly every problem that can fairly be presented to it. If a probable solution consistent with the ordinary course of Nature can be found, we must not invoke an abnormal act of Creative Power"; and in illustration he forthwith proceeded to express his conviction that from time immemorial many worlds of life besides our own have existed, and that "it is not an unscientific hypothesis that life originated on this earth through the moss-grown fragments from the ruins of another world." In spite of the great progress made in science, it is curious to notice the occasional recrudescence of metaphysical dogma. For instance, there is a school which does not hesitate to revive ancient mystifications in order to show that matter and energy can be shattered by philosophical arguments, and have no objective reality. Science is at once more humble and more reverent. She confesses her ignorance of the ultimate nature of matter, of the ultimate nature of energy, and still more of the origin and ultimate synthesis of the two. She is content with her patient investigation of secondary causes, and glad to know that since Tyndall spoke in Belfast she has made great additions to the knowledge of general molecular mechanism, and especially of synthetic artifice in the domain of organic chemistry, though the more exhaustive acquaintance gained only forces us the more to acquiesce in acknowledging the inscrutable mystery of matter. Our conception of the power and potency of matter has grown in little more than a quarter of a century to much more imposing dimensions, and the outlook for the future assuredly suggests the increasing acceleration of our rate of progress. For the impetus he gave to scientific work and thought, and for his fine series of researches chiefly directed to what Newton called the more secret and noble works of Nature within the corpuscles, the world owes Tyndall a debt of gratitude. It is well that his memory should be held in perennial respect, especially in the land of his birth.

#### *The Endowment of Education.*

These are days of munificent benefactions to science and education, which, however, are greater and more numerous in other countries than in our own. Splendid as they are, it may be doubted, if we take into account the change in the value of money, the enormous increase of population and the utility of science to the builders of colossal fortunes, whether they bear comparison with the efforts of earlier days. But the habit of endowing science was so long in practical abeyance that every evidence of its resumption is matter for sincere congratulation. Mr. Cecil Rhodes has dedicated a very large sum of money to the advancement of education, though the means he has chosen are perhaps not the most effective. It must be remembered that his aims were political as much as educational. He had the noble and worthy ambition to promote enduring friendship between the great English-speaking communities of the world, and knowing the strength of college ties he conceived that this end might be greatly furthered by bringing together at an English university the men who would presumably have much to do in later life with the influencing of opinion, or even with the direction of policy. It has been held by some a striking tribute to Oxford that a man but

little given to academic pursuits or modes of thought should think it a matter of high importance to bring men from our colonies or even from Germany, to submit to the formative influences of that ancient seat of learning. But this is perhaps reading Mr. Rhodes backwards. He showed his affectionate recollection of his college days by his gift to Oriel. But, apart from the main idea of fostering good relations between those who will presumably be influential in England, in the colonies, and in the United States, Mr. Rhodes was probably influenced also by the hope that the influx of strangers would help to broaden Oxford notions and to procure revision of conventional arrangements.

Dr. Andrew Carnegie's endowment of Scottish universities, as modified by him in deference to expert advice, is a more direct benefit to the higher education. For while Mr. Rhodes has only enabled young men to get what Oxford has to give, Dr. Carnegie has also enabled his trustees powerfully to augment and improve the teaching equipment of the universities themselves. At the same time, he has provided as far as possible for the enduring usefulness of his money. His trustees form a permanent body external to the universities, which, while possessing no power of direct control, must always, as holder of the purse-strings, be in a position to offer independent and weighty criticisms. More recently Dr. Carnegie has devoted an equal sum of ten million dollars to the foundation of a Carnegie Institution in Washington. Here again he has been guided by the same ideas. He has neither founded a university nor handed over the money to any existing university. He has created a permanent trust charged with the duty of watching educational efforts and helping them from the outside according to the best judgment that can be formed in the circumstances of the moment. Its aims are to be—to promote original research; to discover the exceptional man in every department of study, whether inside or outside of the schools, and to enable him to make his special study his life-work; to increase facilities for higher education; to aid and stimulate the universities and other educational institutions; to assist students who may prefer to study at Washington; and to ensure prompt publication of scientific discoveries. The general purpose of the founder is to secure, if possible, for the United States leadership in the domain of discovery and the utilisation of new forces for the benefit of man. Nothing will more powerfully further this end than attention to the injunction to lay hold of the exceptional man whenever and wherever he may be found, and, having got him, to enable him to carry on the work for which he seems specially designed. That means, I imagine, a scouring of the old world, as well as of the new, for the best men in every department of study—in fact, an assiduous collecting of brains similar to the collecting of rare books and works of art which Americans are now carrying on in so lavish a manner. As in diplomacy and war, so in science, we owe our reputation, and no small part of our prosperity, to exceptional men; and that we do not enjoy these things in fuller measure we owe to our lack of an army of well-trained ordinary men capable of utilising their ideas. Our exceptional men have too often worked in obscurity, without recognition from a public too imperfectly instructed to guess at their greatness, without assistance from a State governed largely by dialecticians, and without help from academic authorities hidebound by the pedantries of medieval scholasticism. For such men we have to wait upon the will of Heaven. Even Dr. Carnegie will not always find them when they are wanted. But what can be done in that direction will be done by institutions like Dr. Carnegie's, and for the benefit of the nation that possesses them in greatest abundance and uses them most intelligently. When contemplating these splendid endowments of learning, it occurred to me that it would be interesting to find out exactly what some definite quantity of scientific achievement has cost in hard cash. In an article by Carl Snyder in the January number of the *North American Review*, entitled "America's Inferior Place in the Scientific World," I found the statement that "it would be hardly too much to say that during the hundred years of its existence the Royal Institution alone has done more for English science than all of the English universities put together. This is certainly true with regard to British industry, for it was here that the discoveries of Faraday were made." I was emboldened by this estimate from a distant and impartial observer to do, what otherwise I might have shrunk from doing, and to take the Royal Institution—after all, the foundation of an American citizen, Count Rumford—as the basis of my inquiry. The work done



at the Royal Institution during the past hundred years is a fairly definite quantity in the mind of every man really conversant with scientific affairs. I have obtained from the books accurate statistics of the total expenditure on experimental inquiry and public demonstrations for the whole of the nineteenth century. The items are:—

Professors' Salaries—Physics and Chemistry	£ 54,600
Laboratory Expenditure .....	24,430
Assistants' Salaries .....	21,590
Total for one hundred years .....	£ 100,620

In addition, the members and friends of the Institution have contributed to a fund for exceptional expenditure for Experimental Research the sum of 9580*l.* It should also be mentioned that a Civil List Pension of 300*l.* was granted to Faraday in 1853, and was continued during twenty-seven years of active work and five years of retirement. Thirty-two years in all, at 300*l.* a year, make a sum of 9600*l.*, representing the national donation, which, added to the amount of expenditure just stated, brings up the total cost of a century of scientific work in the laboratories of the Royal Institution, together with public demonstrations, to 119,800*l.*, or an average of 1200*l.* per annum. I think if you recall the names and achievements of Young, Davy, Faraday, and Tyndall, you will come to the conclusion that the exceptional man is about the cheapest of natural products. It is a popular fallacy that the Royal Institution is handsomely endowed. On the contrary, it has often been in financial straits; and since its foundation by Count Rumford its only considerable bequests have been one from Thomas G. Hodgkins, an American citizen, for Experimental Research, and that of John Fuller for endowing with 95*l.* a year the chairs of Chemistry and Physiology. In this connection the Davy-Faraday Laboratory, founded by the liberality of Dr. Ludwig Mond, will naturally occur to many minds. But though affiliated to the Royal Institution, with, I hope, reciprocal indirect advantages, that Laboratory is financially independent and its endowments are devoted to its own special purpose, which is to provide opportunity to prosecute independent research for worthy and approved applicants of all nationalities. The main reliance of the Royal Institution has always been, and still remains, upon the contributions of its members, and upon corresponding sacrifices in the form of time and labour by its professors. It may be doubted whether we can reasonably count upon a succession of scientific men able and willing to make sacrifices which the conditions of modern life tend to render increasingly burdensome. Modern science is in fact in something of a dilemma. Devotion to abstract research upon small means is becoming always harder to maintain, while at the same time the number of wealthy independent searchers after truth and patrons of science of the style of Joule, Spottiswoode, and De la Rue is apparently becoming smaller. The installations required by the refinements of modern science are continually becoming more costly, so that upon all grounds it would appear that without endowments of the kind provided by Dr. Carnegie the outlook for disinterested research is rather dark. On the other hand, these endowments, unless carefully administered, might obviously tend to impair the single-minded devotion to the search after truth for its own sake to which science has owed almost every memorable advance made in the past. The Carnegie Institute will dispose in a year of as much money as the members of the Royal Institution have expended in a century upon its purely scientific work. It will at least be interesting to note how far the output of high-class scientific work corresponds to the hundredfold application of money to its production. Nor will it be of less interest to the people of this country to observe the results obtained from that moiety of Dr. Carnegie's gift to Scotland which is to be applied to the promotion of scientific research.

*Applied Chemistry, English and Foreign.*

The Diplomatic and Consular reports published from time to time by the Foreign Office are usually too belated to be of much use to business men, but they sometimes contain information concerning what is done in foreign countries which affords food for reflection. One of these reports, issued a year ago, gives a very good account of the German arrangements and provisions for scientific training, and of the enormous commercial demand for the services of men who have passed suc-

cessfully through the universities and Technical High Schools, as well as of the wealth that has accrued to Germany through the systematic application of scientific proficiency to the ordinary business of life.

Taking these points in their order, I have thought it a matter of great interest to obtain a comparative view of chemical equipment in this country and in Germany, and I am indebted to Prof. Henderson of Glasgow, who last year became the secretary of a committee of this Association, of which Prof. Armstrong is chairman, for statistics referring to this country, which enable a comparison to be broadly made. The author of the Consular report estimates that in 1901 there were 4500 trained chemists employed in German works, the number having risen to this point from 1700 employed twenty-five years earlier. It is difficult to give perfectly accurate figures for this country, but a liberal estimate places the number of works chemists at 1500, while at the very outside it cannot be put higher than somewhere between 1500 and 2000. In other words, we cannot show in the United Kingdom, notwithstanding the immense range of the chemical industries in which we once stood prominent, more than one-third of the professional staff employed in Germany. It may perhaps be thought or hoped that we make up in quality for our defect in quantity, but unfortunately this is not the case. On the contrary, the German chemists are, on the average, as superior in technical training and acquirements as they are numerically. Details are given in the report of the training of 633 chemists employed in German works. Of these, 69 per cent. hold the degree of Ph.D., about 10 per cent. hold the diploma of a Technical High School, and about 5 per cent. hold both qualifications. That is to say, 84 per cent. have received a thoroughly systematic and complete chemical training, and 74 per cent. of these add the advantages of a university career. Compare with this the information furnished by 500 chemists in British works. Of these only 21 per cent. are graduates, while about 10 per cent. hold the diploma of a college. Putting the case as high as we can, and ignoring the more practical and thorough training of the German universities, which give their degrees for work done, and not for questions asked and answered on paper, we have only 31 per cent. of systematically trained chemists against 84 per cent. in German works. It ought to be mentioned that about 21 per cent. of the 500 are Fellows or Associates of the Institute of Chemistry, whatever that may amount to in practice, but of these a very large number have already been accounted for under the heads of graduates and holders of diplomas. These figures, which I suspect are much too favourable on the British side, unmistakably point to the prevalence among employers in this country of the antiquated adherence to rule of thumb, which is at the root of much of the backwardness we have to deplore. It hardly needs to be pointed out to such an audience as the present that chemists who are neither graduates of a university, nor holders of a diploma from a technical college, may be competent to carry on existing processes according to traditional methods, but are very unlikely to effect substantial improvements, or to invent new and more efficient processes. I am very far from denying that here and there an individual may be found whose exceptional ability enables him to triumph over all defects of training. But in all educational matters it is the average man whom we have to consider, and the average ability which we have to develop. Now, to take the second point—the actual money value of the industries carried on in Germany by an army of workers both quantitatively and qualitatively so superior to our own. The Consular report estimates the whole value of German chemical industries at not less than fifty millions sterling per annum. These industries have sprung up within the last seventy years, and have received enormous expansion during the last thirty. They are, moreover, very largely founded upon basic discoveries made by English chemists, but never properly appreciated or scientifically developed in the land of their birth. I will place before you some figures showing the growth of a single firm engaged in a single one of these industries—the utilisation of coal tar for the production of drugs, perfumes, and colouring-matters of every conceivable shade. The firm of Friedrich Bayer & Co. employed in 1875, 119 workmen. The number has more than doubled itself every five years, and in May of this year that firm employed 5000 workmen, 160 chemists, 260 engineers and mechanics, and 680 clerks. For many years past it has regularly paid 18 per cent. on the ordinary shares, which this year has risen to 20 per cent.; and in addition, in common

with other and even larger concerns in the same industry, has paid out of profits for immense extensions usually charged to capital account. There is one of these factories the works and plant of which stand in the books at 1,500,000*l.*, while the money actually sunk in them approaches to 5,000,000*l.* In other words, the practical monopoly enjoyed by the German manufacturers enables them to exact huge profits from the rest of the world, and to establish a position which, financially as well as scientifically, is almost unassailable. I must repeat that the fundamental discoveries upon which this gigantic industry is built were made in this country, and were practically developed to a certain extent by their authors. But in spite of the abundance and cheapness of the raw material, and in spite of the evidence that it could be most remuneratively worked up, these men founded no school and had practically no successors. The colours they made were driven out of the field by newer and better colours made from their stuff by the development of their ideas, but these improved colours were made in Germany and not in England. Now what is the explanation of this extraordinary and disastrous phenomenon? I give it in a word—want of education. We had the material in abundance when other nations had comparatively little. We had the capital, and we had the brains, for we originated the whole thing. But we did not possess the diffused education without which the ideas of men of genius cannot fructify beyond the limited scope of an individual. I am aware that our patent laws are sometimes held responsible. Well, they are a contributory cause; but it must be remembered that other nations with patent laws as protective as could be desired have not developed the colour industry. The patent laws have only contributed in a secondary degree, and if the patent laws have been bad, the reason for their badness is again want of education. Make them as bad as you choose, and you only prove that the men who made them, and the public whom these men try to please, were misled by theories instead of being conversant with fact and logic. But the root of the mischief is not in the patent laws or in any legislation whatever. It is in the want of education among our so-called educated classes, and secondarily among the workmen on whom these depend. It is in the abundance of men of ordinary plodding ability, thoroughly trained and methodically directed, that Germany at present has so commanding an advantage. It is the failure of our schools to turn out, and of our manufacturers to demand, men of this kind, which explains our loss of some valuable industries and our precarious hold upon others. Let no one imagine for a moment that this deficiency can be remedied by any amount of that technical training which is now the fashionable nostrum. It is an excellent thing, no doubt, but it must rest upon a foundation of general training. Mental habits are formed for good or evil long before men go to the technical schools. We have to begin at the beginning: we have to train the population from the first to think correctly and logically, to deal at first hand with facts, and to evolve, each one for himself, the solution of a problem put before him, instead of learning by rote the solution given by somebody else. There are plenty of chemists turned out, even by our Universities, who would be of no use to Bayer and Co. They are chock full of formulae, they can recite theories, and they know text-books by heart; but put them to solve a new problem, freshly arisen in the laboratory, and you will find that their learning is all dead. It has not become a vital part of their mental equipment, and they are floored by the first emergence of the unexpected. The men who escape this mental barrenness are men who were somehow or other taught to think long before they went to the university. To my mind, the really appalling thing is not that the Germans have seized this or the other industry, or even that they may have seized upon a dozen industries. It is that the German population has reached a point of general training and specialised equipment which it will take us two generations of hard and intelligently directed educational work to attain. It is that Germany possesses a national weapon of precision which must give her an enormous initial advantage in any and every contest depending upon disciplined and methodised intellect.

#### *History of Cold and the Absolute Zero.*

It was Tyndall's good fortune to appear before you at a moment when a fruitful and comprehensive idea was vivifying the whole domain of scientific thought. At the present time no such broad generalisation presents itself for discussion, while on the other hand the number of specialised studies has enor-

mously increased. Science is advancing in so broad a front by the efforts of so great an army of workers that it would be idle to attempt within the limits of an address to the most indulgent of audiences anything like a survey of chemistry alone. But I have thought it might be instructive, and perhaps not uninteresting to trace briefly in broad outline the development of that branch of study with which my own labours have been recently more intimately connected—a study which I trust I am not too partial in thinking is as full of philosophical interest as of experimental difficulty. The nature of heat and cold must have engaged thinking men from the very earliest dawn of speculation upon the external world; but it will suffice for the present purpose if, disregarding ancient philosophers and even medieval alchemists, we take up the subject where it stood after the great revival of learning, and as it was regarded by the father of the inductive method. That this was an especially attractive subject to Bacon is evident from the frequency with which he recurs to it in his different works, always with lamentation over the inadequacy of the means at disposal for obtaining a considerable degree of cold. Thus in the chapter in the *Natural History*, "*Sylva Sylvarum*," entitled "*Experiments in consort touching the production of cold*," he says, "*The production of cold is a thing very worthy of the inquisition both for the use and the disclosure of causes. For heat and cold are nature's two hands whereby she chiefly worketh, and heat we have in readiness in respect of the fire, but for cold we must stay till it cometh or seek it in deep caves or high mountains, and when all is done we cannot obtain it in any great degree, for furnaces of fire are far hotter than a summer sun, but vaults and hills are not much colder than a winter's frost.*" The great Robert Boyle was the first experimentalist who followed up Bacon's suggestions. In 1682 Boyle read a paper to the Royal Society on "*New Experiments and Observations touching Cold, or an Experimental History of Cold*," published two years later in a separate work. This is really a most complete history of everything known about cold up to that date, but its great merit is the inclusion of numerous experiments made by Boyle himself on frigorific mixtures, and the general effects of such upon matter. The agency chiefly used by Boyle in the conduct of his experiments was the glaciating mixture of snow or ice and salt. In the course of his experiments he made many important observations. Thus he observed that the salts which did not help the snow or ice to dissolve faster gave no effective freezing. He showed that water in becoming ice expands by about one-ninth of its volume, and bursts gun-barrels. He attempted to counteract the expansion and prevent freezing by completely filling a strong iron ball with water before cooling; anticipating that it might burst the bottle by the stupendous force of expansion, or that if it did not, then the ice produced might under the circumstances be heavier than water. He speculated in an ingenious way on the change of water into ice. Thus he says, "*If cold be but a privation of heat through the recess of that ethereal substance which agitated the little eel-like particles of the water and thereby made them compose a fluid body, it may easily be conceived that they should remain rigid in the postures in which the ethereal substance quitted them, and thereby compose an unfluid body like ice; yet how these little eels should by that recess acquire as strong an endeavour outwards as if they were so many little springs and expand themselves with so stupendous a force, is that which does not so readily appear.*" The greatest degree of adventitious cold Boyle was able to produce did not make air exposed to its action lose a full tenth of its own volume, so that, in his own words, the cold does not "*weaken the spring by anything near so considerable as one would expect.*" After making this remarkable observation and commenting upon its unexpected nature, it is strange Boyle did not follow it up. He questions the existence of a body of its own nature supremely cold, by participating in which all other bodies obtain that quality, although the doctrine of a *primum frigidum* had been accepted by many sects of philosophers; for, as he says, "*if a body being cold signify no more than its not having its sensible parts so much agitated as those of our sensorium, it suffices that the sun or the fire or some other agent, whatever it were, that agitated more vehemently its parts before, does either now cease to agitate them or agitates them but very remissly, so that till it be determined whether cold be a positive quality or but a privative it will be needless to contend what particular body ought to be esteemed the *primum frigidum*.*" The whole elaborate investigation cost Boyle immense labour, and he confesses that he "*never handled any part of natural*



philosophy that was so troublesome and full of hardships." He looked upon his results but as a "beginning" in this field of inquiry, and for all the trouble and patience expended he consoled himself with the thought of "men being oftentimes obliged to suffer as much wet and cold and dive as deep to fetch up sponges as to fetch up pearls." After the masterly essay of Boyle, the attention of investigators was chiefly directed to improving thermometrical instruments. The old air thermometer of Galileo being inconvenient to use, the introduction of fluid thermometers greatly aided the inquiry into the action of heat and cold. For a time great difficulty was encountered in selecting proper fixed points on the scales of such instruments, and this stimulated men like Huygens, Newton, Hooke and Amontons to suggest remedies and to conduct experiments. By the beginning of the eighteenth century the freezing-point and the boiling-point of water were agreed upon as fixed points, and the only apparent difficulties to be overcome were the selection of the fluid, accurate calibration of the capillary tube of the thermometer, and a general understanding as to scale divisions. It must be confessed that great confusion and inaccuracy in temperature observations arose from the variety and crudeness of the instruments. This led Amontons in 1702-3 to contribute two papers to the French Academy which reveal great originality in the handling of the subject, and which, strange to say, are not generally known. The first discourse deals with some new properties of the air and the means of accurately ascertaining the temperature in any climate. He regarded heat as due to a movement of the particles of bodies, though he did not in any way specify the nature of the motion involved; and as the general cause of all terrestrial motion, so that in its absence the earth would be without movement in its smallest parts. The new facts he records are observations on the spring or pressure of air brought about by the action of heat. He shows that different masses of air measured at the same initial spring or pressure, when heated to the boiling-point of water, acquire equal increments of spring or pressure, provided the volume of the gas be kept at its initial value. Further, he proves that if the pressure of the gas before heating be doubled or tripled, then the additional spring or pressure resulting from heating to the boiling-point of water is equally doubled or tripled. In other words, the ratio of the total spring of air at two definite and steady temperatures and at constant volume is a constant, independent of the mass or the initial pressure of the air in the thermometer. These results led to the increased perfection of the air thermometer as a standard instrument, Amontons' idea being to express the temperature at any locality in fractions of the degree of heat of boiling water. The great novelty of the instrument is that temperature is defined by the measurement of the length of a column of mercury. In passing, he remarks that we do not know the extreme of heat and cold, but that he has given the results of experiments which establish correspondences for those who wish to consider the subject. In the following year Amontons contributed to the Academy a further paper extending the scope of the inquiry. He there pointed out more explicitly that as the degrees of heat in his thermometer are registered by the height of a column of mercury, which the heat is able to sustain by the spring of the air, it follows that the extreme cold of the thermometer will be that which reduces the air to have no power of spring. This, he says, will be a much greater cold than what we call "very cold," because experiments have shown that if the spring of the air at boiling-point is 73 inches, the degree of heat which remains in the air when brought to the freezing-point of water is still very great, for it can still maintain the spring of  $51\frac{1}{2}$  inches. The greatest climatic cold on the scale of units adopted by Amontons is marked 50, and the greatest summer heat 58, the value for boiling water being 73, and the zero being 52 units below the freezing-point. Thus Amontons was the first to recognise that the use of air as a thermometric substance led to the inference of the existence of a zero of temperature, and his scale is nothing else than the absolute one we are now so familiar with. It results from Amontons' experiments that the air would have no spring left if it were cooled below the freezing-point of water to about  $2\frac{1}{2}$  times the temperature range which separates the boiling-point and the freezing-point. In other words, if we adopt the usual centennial difference between these two points of temperature as 100 degrees, then the zero of Amontons' air thermometer is *minus* 240 degrees. This is a remarkable approximation to our modern value for the same point of *minus* 273 degrees. It has to be confessed that Amontons' valuable

contributions to knowledge met with that fate which has so often for a time overtaken the work of too-advanced discoverers; in other words, it was simply ignored, or in any case not appreciated by the scientific world either of that time or half a century later. It is not till Lambert, in his work on "Pyrometrie" published in 1779, repeated Amontons' experiments and endorsed his results that we find any further reference to the absolute scale or the zero of temperature. Lambert's observations were made with the greatest care and refinement, and resulted in correcting the value of the zero of the air scale to *minus* 270 degrees as compared with Amontons' *minus* 240 degrees. Lambert points out that the degree of temperature which is equal to zero is what one may call absolute cold, and that at this temperature the volume of the air would be practically nothing. In other words, the particles of the air would fall together and touch each other and become dense like water; and from this it may be inferred that the gaseous condition is caused by heat. Lambert says that Amontons' discoveries had found few adherents because they were too beautiful and advanced for the time in which he lived.

About this time a remarkable observation was made by Prof. Braun at Moscow, who, during the severe winter of 1759, succeeded in freezing mercury by the use of a mixture of snow and nitric acid. When we remember that mercury was regarded as quite a peculiar substance possessed of the essential quality of fluidity, we can easily understand the universal interest created by the experiment of Braun. This was accentuated by the observations he made on the temperature given by the mercury thermometer, which appeared to record a temperature as low as *minus* 200° C. The experiments were soon repeated by Hutchins at Hudson's Bay, who conducted his work with the aid of suggestions given him by Cavendish and Black. The result of the new observations was to show that the freezing-point of mercury is only *minus* 40° C., the errors in former experiments having been due to the great contraction of the mercury in the thermometer in passing into the solid state. From this it followed that the enormous natural and artificial colds which had generally been believed in had no proved existence. Still the possible existence of a zero of temperature very different from that deduced from gas thermometry had the support of such distinguished names as those of Laplace and Lavoisier. In their great memoir on "Heat," after making what they consider reasonable hypotheses as to the relation between specific heat and total heat, they calculate values for the zero which range from 1500° to 3000° below melting ice. On the whole, they regard the absolute zero as being in any case 600° below the freezing-point. Lavoisier, in his "Elements of Chemistry" published in 1792, goes further in the direction of indefinitely lowering the zero of temperature when he says, "We are still very far from being able to produce the degree of absolute cold, or total deprivation of heat, being unacquainted with any degree of coldness which we cannot suppose capable of still further augmentation; hence it follows we are incapable of causing the ultimate particles of bodies to approach each other as near as possible, and thus these particles do not touch each other in any state hitherto known." Even as late as the beginning of the nineteenth century we find Dalton, in his new system of "Chemical Philosophy," giving ten calculations of this value, and adopting finally as the natural zero of temperature *minus* 3000° C.

In Black's lectures we find that he takes a very cautious view with regard to the zero of temperature, but as usual is admirably clear with regard to its exposition. Thus he says, "We are ignorant of the lowest possible degree or beginning of heat. Some ingenious attempts have been made to estimate what it may be, but they have not proved satisfactory. Our knowledge of the degrees of heat may be compared to what we should have of a chain the two ends of which were hidden from us and the middle only exposed to our view. We might put distinct marks on some of the links, and number the rest according as they are nearest to or further removed from the principal links; but not knowing the distance of any links from the end of the chain we could not compare them together with respect to their distance, or say that one link was twice as far from the end of the chain as another." It is interesting to observe, however, that Black was evidently well acquainted with the work of Amontons, and strongly supports his inference as to the nature of air. Thus, in discussing the general cause of vaporisation, Black says that some philoso-

phers have adopted the view "that every palpable elastic fluid in nature is produced and preserved in this form by the action of heat. Mr. Amontons, an ingenious member of the late Royal Academy of Sciences, at Paris, was the first who proposed this idea with respect to the atmosphere. He supposed that it might be deprived of the whole of its elasticity and condensed and even frozen into a solid matter were it in our power to apply to it a sufficient cold; that it is a substance that differs from others by being incomparably more volatile, and which is therefore converted into vapour and preserved in that form by a weaker heat than any that ever happened or can obtain in this globe, and which therefore cannot appear under any other form than the one it now wears, so long as the constitution of the world remains the same as at present." The views that Black attributes to Amontons have been generally associated with the name of Lavoisier, who practically admitted similar possibilities as to the nature of air; but it is not likely that in such matters Black would commit any mistake as to the real author of a particular idea, especially in his own department of knowledge. Black's own special contribution to low-temperature studies was his explanation of the interaction of mixtures of ice with salts and acids by applying the doctrine of the latent heat of fluidity of ice to account for the frigorific effect. In a similar way, Black explained the origin of the cold produced in Cullen's remarkable experiment of the evaporation of ether under the receiver of an air-pump by pointing out that the latent heat of vaporisation in this case necessitated such a result. Thus, by applying his own discoveries of latent heat, Black gave an intelligent explanation of the cause of all the low-temperature phenomena known in his day.

After the gaseous laws had been definitely formulated by Gay-Lussac and Dalton, the question of the absolute zero of temperature, as deduced from the properties of gases, was revived by Clement and Desormes. These distinguished investigators presented a paper on the subject to the French Academy in 1812, which, it appears, was rejected by that body. The authors subsequently elected to publish it in 1819. Relying on what we know now to have been a faulty hypothesis, they deduced from observations on the heating of air rushing into a vacuum the temperature of *minus* 267 degrees as that of the absolute zero. They further endeavoured to show, by extending to lower temperatures the volume or the pressure coefficients of gases given by Gay-Lussac, that at the same temperature of *minus* 267 degrees the gases would contract so as to possess no appreciable volume, or, alternatively, if the pressure was under consideration, it would become so small as to be non-existent. Although full reference is given to previous work bearing on the same subject, yet, curiously enough, no mention is made of the name of Amontons. It certainly gave remarkable support to Amontons' notion of the zero to find that simple gases like hydrogen and compound gases like ammonia, hydrochloric, carbonic and sulphurous acids should all point to substantially the same value for this temperature. But the most curious fact about this research of Clement and Desormes is that Gay-Lussac was a bitter opponent of the validity of the inferences they drew either from his work or their own. The mode in which Gay-Lussac regarded the subject may be succinctly put as follows: A quick compression of air to one-fifth volume raises its temperature to 300 degrees, and if this could be made much greater and instantaneous the temperature might rise to 1000 or 2000 degrees. Conversely, if air under five atmospheres were suddenly dilated, it would absorb as much heat as it had evolved during compression, and its temperature would be lowered by 300 degrees. Therefore, if air were taken and compressed to fifty atmospheres or more, the cold produced by its sudden expansion would have no limit. In order to meet this position, Clement and Desormes adopted the following reasoning: They pointed out that it had not been proved that Gay-Lussac was correct in his hypothesis, but that in any case it tacitly involves the assumption that a limited quantity of matter possesses an unlimited supply of heat. If this were the case, then heat would be unlike any other measurable thing or quality. It is, therefore, more consistent with the course of nature to suppose that the amount of heat in a body is like the quantity of elastic fluid filling a vessel, which, while definite in original amount, one may make less and less by getting nearer to a complete exhaustion. Further, to realise the absolute zero in the one case is just as impossible as to realise the absolute vacuum in the other; and as we do not doubt a zero of pressure, although it is unattain-

able, for the same reason we ought to accept the reality of the absolute zero. We know now that Gay-Lussac was wrong in supposing the increment of temperature arising from a given gaseous compression would produce a corresponding decrement from an identical expansion. After this time the zero of temperature was generally recognised as a fixed ideal point, but in order to show that it was hypothetical a distinction was drawn between the use of the expressions, zero of absolute temperature and the absolute zero.

The whole question took an entirely new form when Lord Kelvin, in 1848, after the mechanical equivalent of heat had been determined by Joule, drew attention to the great principles underlying Carnot's work on the "Motive Power of Heat," and applied them to an absolute method of temperature measurement, which is completely independent of the properties of any particular substance. The principle was that for a difference of one degree on this scale, between the temperatures of the source and refrigerator, a perfect engine should give the same amount of work in every part of the scale. Taking the same fixed points as for the Centigrade scale, and making 100 of the new degrees cover that range, it was found that the degrees not only within that range, but as far beyond as experimental data supplied the means of comparison, differed by only minute quantities from those of Regnault's air thermometer. The zero of the new scale had to be determined by the consideration that when the refrigerator was at the zero of temperature the perfect engine should give an amount of work equal to the full mechanical equivalent of the heat taken up. This led to a zero of 273 degrees below the temperature of freezing water, substantially the same as that deduced from a study of the gaseous state. It was a great advance to demonstrate by the application of the laws of thermodynamics not only that the zero of temperature is a reality, but that it must be located at 273 degrees below the freezing-point of water. As no one has attempted to impugn the solid foundation of theory and experiment on which Lord Kelvin based his thermodynamic scale, the existence of a definite zero of temperature must be acknowledged as a fundamental scientific fact.

#### *Liquefaction of Gases and Continuity of State.*

In these speculations, however, chemists were dealing theoretically with temperatures to which they could not make any but the most distant experimental approach. Cullen, the teacher of Black, had indeed shown how to lower temperature by the evaporation of volatile bodies, such as ether, by the aid of the air-pump, and the later experiments of Leslie and Wollaston extended the same principle. Davy and Faraday made the most of the means at command in liquefying the more condensable gases, while at the same time Davy pointed out that they in turn might be utilised to procure greater cold by their rapid reversion into the æriform state. Still the chemist was sorely hampered by the want of some powerful and accessible agent for the production of temperatures much lower than had ever been attained. That want was supplied by Thilorier, who in 1835 produced liquid carbonic acid in large quantities, and further made the fortunate discovery that the liquid could be frozen into a snow by its own evaporation. Faraday was prompt to take advantage of this new and potent agent. Under exhaustion he lowered its boiling-point from *minus* 78° C. to *minus* 110° C., and by combining this low temperature with pressure all the gases were liquefied by the year 1844, with the exception of the three elementary gases—hydrogen, nitrogen, and oxygen, and three compound gases—carbonic oxide, marsh gas, and nitric oxide; Andrews some twenty-five years after the work of Faraday attempted to induce change of state in the uncondensed gases by using much higher pressures than Faraday employed. Combining the temperature of a solid carbonic acid bath with pressures of 300 atmospheres, Andrews confirmed the earlier work of Natterer by showing that the gases become proportionately less compressible with growing pressure. While such investigations were proceeding, Regnault and Magnus had completed their refined investigations on the laws of Boyle and Gay-Lussac. A very important series of experiments was made by Joule and Kelvin "On the Thermal Effects of Fluids in Motion" about 1862, in which the thermometrical effects of passing gases under compression through porous plugs furnished important data for the study of the mutual action of the gas



molecules. No one, however, had attempted to make a complete study of a liquefiable gas throughout wide ranges of temperature. This was accomplished by Andrews in 1869, and his Bakerian Lecture "On the Continuity of the Gaseous and Liquid States of Matter" will always be regarded as an epoch-making investigation. During the course of this research Andrews observed that liquid carbonic acid raised to a temperature of  $31^{\circ}\text{C}$ . lost the sharp concave surface of demarcation between the liquid and the gas, the space being now occupied by a homogeneous fluid which exhibited, when the pressure was suddenly diminished or the temperature slightly lowered, a peculiar appearance of moving or flickering striæ, due to great local alterations of density. At temperatures above  $31^{\circ}\text{C}$ . the separation into two distinct kinds of matter could not be effected even when the pressure reached 400 atmospheres. This limiting temperature of the change of state from gas to liquid Andrews called the critical temperature. He showed that this temperature is constant, and differs with each substance, and that it is always associated with a definite pressure peculiar to each body. Thus the two constants, critical temperature and pressure, which have been of the greatest importance in subsequent investigations, came to be defined, and a complete experimental proof was given that "the gaseous and liquid states are only two distinct stages of the same condition of matter and are capable of passing into one another by a process of continuous change."

In 1873 an essay "On the Continuity of the Gaseous and Liquid State," full of new and suggestive ideas, was published by van der Waals, who, recognising the value of Clausius' new conception of the Virial in Dynamics, for a long-continued series of motions, either oscillatory or changing exceedingly slowly with time, applied it to the consideration of the molecular movements of the particles of the gaseous substance, and after much refined investigation, and the fullest experimental calculation available at the time, devised his well-known Equation of Continuity. Its paramount merit is that it is based entirely on a mechanical foundation, and is in no sense empiric; we may therefore look upon it as having a secure foundation in fact, but as being capable of extension and improvement. James Thomson, realising that the straight-line breach of continuous curvature in the Andrews isothermals was untenable to the physical mind, propounded his emendation of the Andrews curves—namely, that they were continuous and of S form. We also owe to James Thomson the conception and execution of a three-dimensional model of Andrews' results, which has been of the greatest service in exhibiting the three variables by means of a specific surface afterwards greatly extended and developed by Prof. Willard Gibbs. The suggestive work of James Thomson undoubtedly was a valuable aid to van der Waals, for as soon as he reached the point where his equation had to show the continuity of the two states this was the first difficulty he had to encounter, and he succeeded in giving the explanation. He also gave a satisfactory reason for the existence of a minimum value of the product of volume and pressure in the Regnault isothermals. His isothermals, with James Thomson's completion of them, were now shown to be the results of the laws of dynamics. Andrews applied the new equation to the consideration of the coefficients of expansion with temperature and of pressure with temperature, showing that although they were nearly equal, nevertheless they were almost independent quantities. His investigation of the capillarity constant was masterly, and he added further to our knowledge of the magnitudes of the molecules of gases and of their mean free paths. Following up the experiments of Joule and Kelvin, he showed how their cooling coefficients could be deduced, and proved that they vanished at a temperature in each case which is a constant multiple of the specific critical temperature. The equation of continuity developed by van der Waals involved the use of three constants instead of one, as in the old law of Boyle and Charles, the latter being only utilised to express the relation of temperature, pressure, and volume, when the gas is far removed from its point of liquefaction. Of the two new constants one represents the molecular pressure arising from the attraction between the molecules, the other four times the volume of the molecules. Given these constants of a gas, van der Waals showed that his equation not only fitted into the general characters of the isothermals, but also gave the values of the critical temperature, the critical pressure and the critical volume. In the case of carbonic acid the theoretical results were found to be in remarkable agreement with the experimental values of Andrews. This gave chemists the means of ascertain-

ing the critical constants, provided sufficiently accurate data derived from the study of a few properly distributed isothermals of the gaseous substance were available. Such important data came into the possession of chemists when Amagat published his valuable paper on "The Isothermals of Hydrogen, Nitrogen, Oxygen, Ethylene, &c.," in the year 1880. It now became possible to calculate the critical data with comparative accuracy for the so-called permanent gases oxygen and nitrogen, and this was done by Sarrau in 1882. In the meantime a great impulse had been given to a further attack upon the so-called permanent gases by the suggestive experiments made by Pictet and Cailletet. The static liquefaction of oxygen was effected by Wroblewski in 1883, and thereby the theoretical conclusions derived from van der Waals' equation were substantially confirmed. The liquefaction of oxygen and air was achieved through the use of liquid ethylene as a cooling agent, which enabled a temperature of *minus* 140 degrees to be maintained by its steady evaporation *in vacuo*. From this time liquid oxygen and air came to be regarded as the potential cooling agents for future research, commanding as they did a temperature of 200 degrees below melting ice. The theoretical side of the question received at the hands of van der Waals a second contribution, which was even more important than his original essay, and that was his novel and ingenious development of what he calls "The Theory of Corresponding States." He defined the corresponding states of two substances as those in which the ratios of the temperature, pressure and volume to the critical temperature, pressure and volume respectively were the same for the two substances, and in corresponding states he showed that the three pairs of ratios all coincided. From this a series of remarkable propositions was developed, some new, some proving previous laws that were hitherto only empiric, and some completing and correcting faulty though approximate laws. As examples, he succeeded in calculating the boiling-point of carbonic acid from observations on ether vapour, proved Kopp's law of molecular volumes, and showed that at corresponding temperatures the molecular latent heats of vaporisation are proportional to the absolute critical temperature, and that under the same conditions the coefficients of liquid expansion are inversely proportional to the absolute critical temperature, and that the coefficients of liquid compressibility are inversely proportional to the critical pressure. All these propositions and deductions are in the main correct, though further experimental investigation has shown minor discrepancies requiring explanation. Various proposals have been made to supplement van der Waals' equation so as to bring it into line with experiments, some being entirely empiric, others theoretical. Clausius, Sarrau, Wroblewski, Battelli, and others attacked the question empirically, and in the main preserved the co-volume (depending on the total volume of the molecules) unaltered while trying to modify the constant of molecular attraction. Their success depended entirely on the fact that, instead of limiting the number of constants to three, some of them have increased them to as many as ten. On the other hand, a series of very remarkable theoretical investigations has been made by van der Waals himself, by Kammerlingh Onnes, Korteweg, Jaeger, Boltzmann, Dieterici, and Riengannum, and others, all directed in the main towards an admitted variation in the value of the co-volume while preserving the molecular attraction constant. The theoretical reductions of Tait lead to the conclusion that a substance below its critical point ought to have two different equations of the van der Waals type, one referring to the liquid and the other to the gaseous phase. One important fact was soon elicited—namely, that the law of correspondence demanded only that the equation should contain not more than three constants for each body. The simplest extension is that made by Reinganum, in which he increased the pressure for a given mean kinetic energy of the particles inversely in the ratio of the diminution of free volume, due to the molecules possessing linear extension. Berthelot has shown how a "reduced" isothermal may be got by taking two other prominent points as units of measurement instead of the critical coordinates. The most suggestive advance in the improvement of the van der Waals equation has been made by a lady, Mme. Christine Meyer. The idea at the base of this new development may be understood from the following general statement: van der Waals brings the van der Waals surfaces for all substances into coincidence at the point where volume, pressure and temperature are nothing, and then stretches or compresses all the surfaces parallel to the three axes of volume,

pressure and temperature until their critical points coincide. But on this plan the surfaces do not quite coincide, because the points where the three variables are respectively nothing are not corresponding points. Mme. Meyer's plan is to bring all the critical points first into coincidence, and then to compress or extend all the representative surfaces parallel to the three axes of volume, pressure and temperature until the surfaces coincide. In this way, taking twenty-nine different substances, she completely verifies from experiment van der Waals' law of correspondence. The theory of van der Waals has been one of the greatest importance in directing experimental investigation and in attacking the difficult problems of the liquefaction of the most permanent gases. One of its greatest triumphs has been the proof that the critical constants and the boiling-point of hydrogen theoretically deduced by Wroblewski from a study of the isothermals of the gas taken far above the temperature of liquefaction are remarkably near the experimental values. We may safely infer, therefore, that if hereafter a gas be discovered in small quantity even four times more volatile than liquid hydrogen, yet by a study of its isothermals at low temperature we shall succeed in finding its most important liquid constants, although the isolation of the real liquid may for the time be impossible. It is perhaps not too much to say that, as a prolific source of knowledge in the department dealing with the continuity of state in matter, it would be necessary to go back to Carnot's cycle to find a proposition of greater importance than the theory of van der Waals and his development of the law of corresponding states.

It will be apparent from what has just been said that, thanks to the labours of Andrews, van der Waals, and others, theory had again far outrun experiment. We could calculate the constants and predict some of the simple physical characteristics of liquid oxygen, hydrogen or nitrogen with a high degree of confidence long before any one of the three had been obtained in the static liquid condition permitting of the experimental verification of the theory. This was the more tantalising, because, with whatever confidence the chemist may anticipate the substantial corroboration of his theory, he also anticipates with almost equal conviction that, as he approaches more and more nearly to the zero of absolute temperature, he will encounter phenomena compelling modification, revision and refinement of formulas which fairly covered the facts previously known. Just as nearly seventy years ago chemists were waiting for some means of getting a temperature of 100 degrees below melting ice, so ten years ago they were casting about for the means of going 100 degrees lower still. The difficulty, it need hardly be said, increases in a geometrical rather than in an arithmetical ratio. Its magnitude may be estimated from the fact that to produce liquid air in the atmosphere of an ordinary laboratory is a feat analogous to the production of liquid water starting from steam at a white heat, and working with all the implements and surroundings at the same high temperature. The problem was not so much how to produce intense cold as how to save it when produced from being immediately levelled up by the relatively superheated surroundings. Ordinary non-conducting packings were inadmissible because they are both cumbersome and opaque, while in working near the limits of our resources it is essential that the product should be visible and readily handled. It was while puzzling over this mechanical and manipulative difficulty in 1892 that it occurred to me that the principle of an arrangement used nearly twenty years before in some calorimetric experiments, which was based upon the work of Dulong and Petit on radiation, might be employed with advantage as well to protect old substances from heat as hot ones from rapid cooling. I therefore tried the effect of keeping liquefied gases in vessels having a double wall, the annular space between being very highly exhausted. Experiments showed that liquid air evaporated at only one-fifth of the rate prevailing when it was placed in a similar unexhausted vessel, owing to the convective transference of heat by the gas particles being enormously reduced by the high vacuum. But, in addition, these vessels lend themselves to an arrangement by which radiant heat can also be cut off. It was found that when the inner walls were coated with a bright deposit of silver the influx of heat was diminished to one-sixth the amount entering without the metallic coating. The total effect of the high vacuum and the silvering is to reduce the ingoing heat to about 3 per cent. The efficiency of such vessels depends upon getting as high a vacuum as possible, and cold is one of the best means of effecting the desired exhaustion.

All that is necessary is to fill completely the space that has to be exhausted with an easily condensable vapour, and then to freeze it out in a receptacle attached to the primary vessel that can be sealed off. The advantage of this method is that no air-pump is required, and that theoretically there is no limit to the degree of exhaustion that can be obtained. The action is rapid, provided liquid air is the cooling agent, and vapours like mercury, water or benzol are employed. It is obvious that when we have to deal with such an exceptionally volatile liquid as hydrogen, the vapour filling may be omitted because air itself is now an easily condensable vapour. In other words, liquid hydrogen, collected in such vessels with the annular space full of air, immediately solidifies the air and thereby surrounds itself with a high vacuum. In the same way, when it shall be possible to collect a liquid boiling on the absolute scale at about 5 degrees, as compared with the 20 degrees of hydrogen, then you might have the annular space filled with the latter gas to begin with, and yet get directly a very high vacuum, owing to the solidification of the hydrogen. Many combinations of vacuum vessels can be arranged, and the lower the temperature at which we have to operate the more useful they become. Vessels of this kind are now in general use, and in them liquid air has crossed the American continent. Of the various forms, that variety is of special importance which has a spiral tube joining the bottom part of the walls, so that any liquid gas may be drawn off from the interior of such a vessel. In the working of regenerative coils such a device becomes all-important, and such special vessels cannot be dispensed with for the liquefaction of hydrogen.

In the early experiments of Pictet and Cailletet, cooling was produced by the sudden expansion of the highly compressed gas preferably at a low temperature, the former using a jet that lasted for some time, the latter an instantaneous adiabatic expansion in a strong glass tube. Neither process was practicable as a mode of producing liquid gases, but both gave valuable indications of partial change into the liquid state by the production of a temporary mist. Linde, however, saw that the continuous use of a jet of highly compressed gas, combined with regenerative cooling, must lead to liquefaction on account of what is called the Kelvin-Joule effect; and he succeeded in making a machine, based on this principle, capable of producing liquid air for industrial purposes. These experimenters had proved that, owing to molecular attraction, compressed gases passing through a porous plug or small aperture were lowered in temperature by an amount depending on the difference of pressure, and inversely as the square of the absolute temperature. This means that for a steady difference of pressure the cooling is greater the lower the temperature. The only gas that did not show cooling under such conditions was hydrogen. Instead of being cooled it became actually hotter. The reason for this apparent anomaly in the Kelvin-Joule effect is that every gas has a thermometric point of inversion above which it is heated and below which it is cooled. This inversion point, according to van der Waals, is six and three-quarter times the critical point. The efficiency of the Linde process depends on working with highly compressed gas well below the inversion temperature, and in this respect this point may be said to take the place of the critical one, when in the ordinary way direct liquefaction is being effected by the use of specific liquid cooling agents. The success of both processes depends upon working within a certain temperature range, only the Linde method gives us a much wider range of temperature within which liquefaction can be effected. This is not the case if, instead of depending on getting cooling by the internal work done by the attraction of the gas molecules, we force the compressed gas to do external work as in the well-known air machines of Kirk and Coleman. Both these inventors have pointed out that there is no limit of temperature, short of liquefaction of the gas in use in the circuit, that such machines are not capable of giving. While it is theoretically clear that such machines ought to be capable of maintaining the lowest temperatures, and that with the least expenditure of power, it is a very different matter to overcome the practical difficulties of working such machines under the conditions. Coleman kept a machine delivering air at *minus* 83 degrees for hours, but he did not carry his experiments any further. Recently Monsieur Claude, of Paris, has, however, succeeded in working a machine of this type so efficiently that he has managed to produce one litre of liquid air per horse power expended per hour in the running of the engine. This output is twice as good as that given by the Linde machine,



and there is no reason to doubt that the yield will be still further improved. It is clear, therefore, that in the immediate future the production of liquid air and hydrogen will be effected most economically by the use of machines producing cold by the expenditure of mechanical work.

#### *Liquid Hydrogen and Helium.*

To the physicist the copious production of liquid air by the methods described was of peculiar interest and value as affording the means of attacking the far more difficult problem of the liquefaction of hydrogen, and even as encouraging the hope that liquid hydrogen might in time be employed for the liquefaction of yet more volatile elements, apart from the importance which its liquefaction must hold in the process of the steady advance towards the absolute zero. Hydrogen is an element of especial interest, because the study of its properties and chemical relations led great chemists like Faraday, Dumas, Daniel, Graham and Andrews to entertain the view that if it could ever be brought into the state of liquid or solid it would reveal metallic characters. Looking to the special chemical relations of the combined hydrogen in water, alkaline oxides, acids and salts, together with the behaviour of these substances on electrolysis, we are forced to conclude that hydrogen behaves as the analogue of a metal. After the beautiful discovery of Graham that palladium can absorb some hundreds of times its own volume of hydrogen and still retain its lustre and general metallic character, the impression that hydrogen was probably a member of the metallic group became very general. The only chemist who adopted another view was my distinguished predecessor, Prof. Odling. In his "Manual of Chemistry," published in 1861, he pointed out that hydrogen has chlorous as well as basic relations, and that they are as decided, important, and frequent as its other relations. From such considerations he arrived at the conclusion that hydrogen is essentially a neutral or intermediate body, and therefore we should not expect to find liquid or solid hydrogen possess the appearance of a metal. This extraordinary prevision, so characteristic of Odling, was proved to be correct some thirty-seven years after it was made. Another curious anticipation was made by Dumas in a letter addressed to Pictet, in which he says that the metal most analogous to hydrogen is magnesium and that probably both elements have the same atomic volume, so that the density of hydrogen, for this reason, would be about the value elicited by subsequent experiments. Later on, in 1872, when Newlands began to arrange the elements in periodic groups, he regarded hydrogen as the lowest member of the chlorine family; but Mendeleëff in his later classification placed hydrogen in the group of the alkaline metals; on the other hand, Dr. Johnstone Stoney classes hydrogen with the alkaline earth metals and magnesium. From this speculative divergency it is clear no definite conclusion could be reached regarding the physical properties of liquid or solid hydrogen, and the only way to arrive at the truth was to prosecute low-temperature research until success attended the efforts to produce its liquefaction. This result I definitely obtained in 1898. The case of liquid hydrogen is, in fact, an excellent illustration of the truth already referred to, that no theoretical forecast, however apparently justified by analogy, can be finally accepted as true until confirmed by actual experiment. Liquid hydrogen is a colourless, transparent body of extraordinary intrinsic interest. It has a clearly defined surface, is easily seen, drops well, in spite of the fact that its surface tension is only the thirty-fifth part of that of water, or about one-fifth that of liquid air, and can be poured easily from vessel to vessel. The liquid does not conduct electricity, and, if anything, is slightly diamagnetic. Compared with an equal volume of liquid air, it requires only one-fifth the quantity of heat for vaporisation; on the other hand, its specific heat is ten times that of liquid air or five times that of water. The coefficient of expansion of the fluid is remarkable, being about ten times that of gas; it is by far the lightest liquid known to exist, its density being only one-fourteenth that of water; the lightest liquid previously known was liquid marsh gas, which is six times heavier. The only solid which has so small density as to float upon its surface is a piece of pith wood. It is by far the coldest liquid known. At ordinary atmospheric pressure it boils at *minus* 252.5 degrees or 20.5 degrees absolute. The critical point of the liquid is about 29 degrees absolute and the critical pressure not more than fifteen atmospheres. The vapour of the hydrogen arising from the liquid has nearly the density of air—that is, it is fourteen

times that of the gas at the ordinary temperature. Reduction of the pressure by an air-pump brings down the temperature to *minus* 258 degrees, when the liquid becomes a solid resembling frozen foam, and this by further exhaustion is cooled to *minus* 260 degrees, or 13 degrees absolute, which is the lowest steady temperature that has been reached. The solid may also be got in the form of a clear, transparent ice, melting at about 15 degrees absolute, under a pressure of 55 mm., possessing the unique density of one-eleventh that of water. Such cold involves the solidification of every gaseous substance but one that is at present definitely known to the chemist, and so liquid hydrogen introduces the investigator to a world of solid bodies. The contrast between this refrigerating substance and liquid air is most remarkable. On the removal of the loose plug of cotton-wool used to cover the mouth of the vacuum vessel in which it is stored, the action is followed by a miniature snow-storm of solid air, formed by the freezing of the atmosphere at the point where it comes into contact with the cold vapour rising from the liquid. This solid air falls into the vessel and accumulates as a white snow at the bottom of the liquid hydrogen. When the outside of an ordinary test-tube is cooled by immersion in the liquid, it is soon observed to fill up with solid air, and if the tube be now lifted out a double effect is visible, for liquid air is produced both in the inside and on the outside of the tube—in the one case by the melting of the solid, and in the other by condensation from the atmosphere. A tuft of cotton-wool soaked in the liquid and then held near the pole of a strong magnet is attracted, and it might be inferred therefrom that liquid hydrogen is a magnetic body. This, however, is not the case: the attraction is due neither to the cotton-wool nor to the hydrogen—which indeed evaporates almost as soon as the tuft is taken out of the liquid—but to the oxygen of the air, which is well known to be a magnetic body, frozen in the wool by the extreme cold.

The strong condensing powers of liquid hydrogen afford a simple means of producing vacua of very high tenuity. When one end of a sealed tube containing ordinary air is placed for a short time in the liquid, the contained air accumulates as a solid at the bottom, while the higher part is almost entirely deprived of particles of gas. So perfect is the vacuum thus formed, that the electric discharge can be made to pass only with the greatest difficulty. Another important application of liquid air, liquid hydrogen, &c., is as analytic agents. Thus, if a gaseous mixture be cooled by means of liquid oxygen, only those constituents will be left in the gaseous state which are less condensable than oxygen. Similarly, if this gaseous residue be in its turn cooled in liquid hydrogen, a still further separation will be effected, everything that is less volatile than hydrogen being condensed to a liquid or solid. By proceeding in this fashion it has been found possible to isolate helium from a mixture in which it is present to the extent of only one part in one thousand. By the evaporation of solid hydrogen under the air-pump we can reach within 13 or 14 degrees of the zero, but there or thereabouts our progress is barred. This gap of 13 degrees might seem at first sight insignificant in comparison with the hundreds that have already been conquered. But to win one degree low down the scale is quite a different matter from doing so at higher temperatures; in fact, to annihilate these few remaining degrees would be a far greater achievement than any so far accomplished in low-temperature research. For the difficulty is twofold, having to do partly with process and partly with material. The application of the methods used in the liquefaction of gases becomes continually harder and more troublesome as the working temperature is reduced; thus, to pass from liquid air to liquid hydrogen—a difference of 60 degrees—is, from a thermodynamic point of view, as difficult as to bridge the gap of 150 degrees that separates liquid chlorine and liquid air. By the use of a new liquid gas exceeding hydrogen in volatility to the same extent as hydrogen does nitrogen, the investigator might get to within five degrees of the zero; but even a second hypothetical substance, again exceeding the first one in volatility to an equal extent, would not suffice to bring him quite to the point of his ambition. That the zero will ever be reached by man is extremely improbable. A thermometer introduced into regions outside the uttermost confines of the earth's atmosphere might approach the absolute zero, provided that its parts were highly transparent to all kinds of radiation, otherwise it would be affected by the radiation of the sun, and would therefore become heated. But supposing all difficulties to be overcome,

and the experimenter to be able to reach within a few degrees of the zero, it is by no means certain that he would find the near approach of the death of matter sometimes pictured. Any forecast of the phenomena that would be seen must be based on the assumption that there is continuity between the processes studied at attainable temperatures and those which take place at still lower ones. Is such an assumption justified? It is true that many changes in the properties of substances have been found to vary steadily with the degree of cold to which they are exposed. But it would be rash to take for granted that the changes which have been traced in explored regions continue to the same extent and in the same direction in those which are as yet unexplored. Of such a breakdown low-temperature research has already yielded a direct proof at least in one case. A series of experiments with pure metals showed that their electrical resistance gradually decreases as they are cooled to lower and lower temperatures, in such ratio that it appeared probable that at the zero of absolute temperature they would have no resistance at all and would become perfect conductors of electricity. This was the inference that seemed justifiable by observations taken at depths of cold which can be obtained by means of liquid air and less powerful refrigerants. But with the advent of the more powerful refrigerant liquid hydrogen it became necessary to revise that conclusion. A discrepancy was first observed when a platinum resistance thermometer was used to ascertain the temperature of that liquid boiling under atmospheric and reduced pressure. All known liquids, when forced to evaporate quickly by being placed in the exhausted receiver of an air-pump, undergo a reduction in temperature, but when hydrogen was treated in this way it appeared to be an exception. The resistance thermometer showed no reduction as was expected, and it became a question whether it was the hydrogen or the thermometer that was behaving abnormally. Ultimately, by the adoption of other thermometrical appliances, the temperature of the hydrogen was proved to be lowered by exhaustion as theory indicated. Hence it was the platinum thermometer which had broken down; in other words, the electrical resistance of the metal employed in its construction was not, at temperatures about *minus* 250° C., decreased by cold in the same proportion as at temperatures about *minus* 200°. This being the case, there is no longer any reason to suppose that at the absolute zero platinum would become a perfect conductor of electricity; and in view of the similarity between the behaviour of platinum and that of other pure metals in respect of temperature and conductivity, the presumption is that the same is true of them also. At any rate, the knowledge that in the case of at least one property of matter we have succeeded in attaining a depth of cold sufficient to bring about unexpected change in the law expressing the variation of that property with temperature, is sufficient to show the necessity for extreme caution in extending our inferences regarding the properties of matter near the zero of temperature. Lord Kelvin evidently anticipates the possibility of more remarkable electrical properties being met with in the metals near the zero. A theoretical investigation on the relation of "electrons" and atoms has led him to suggest a hypothetical metal having the following remarkable properties: below 1 degree absolute it is a perfect insulator of electricity, at 2 degrees it shows noticeable conductivity, and at 6 degrees it possesses high conductivity. It may safely be predicted that liquid hydrogen will be the means by which many obscure problems of physics and chemistry will ultimately be solved, so that the liquefaction of the last of the old permanent gases is as pregnant now with future consequences of great scientific moment as was the liquefaction of chlorine in the early years of the last century.

The next step towards the absolute zero is to find another gas more volatile than hydrogen, and that we possess in the gas occurring in cleveite, identified by Ramsay as helium, a gas which is widely distributed, like hydrogen, in the sun, stars and nebulae. A specimen of this gas was subjected by Olszewski to liquid air temperatures, combined with compression and subsequent expansion, following the Cailletet method, and resulted in his being unable to discover any appearance of liquefaction, even in the form of mist. His experiments led him to infer that the boiling-point of the substance is probably below 9 degrees absolute. After Lord Rayleigh had found a new source of helium in the gases which are derived from the Bath springs, and liquid hydrogen became available as a cooling agent, a

specimen of helium cooled in liquid hydrogen showed the formation of fluid, but this turned out to be owing to the presence of an unknown admixture of other gases. As a matter of fact, a year before the date of this experiment I had recorded indications of the presence of unknown gases in the spectrum of helium derived from this source. When subsequently such condensable constituents were removed, the purified helium showed no signs of liquefaction, even when compressed to 80 atmospheres, while the tube containing it was surrounded with solid hydrogen. Further, on suddenly expanding, no instantaneous mist appeared. Thus helium was definitely proved to be a much more volatile substance than hydrogen in either the liquid or solid condition. The inference to be drawn from the adiabatic expansion effected under the circumstances is that helium must have touched a temperature of from 9 to 10 degrees for a short time without showing any signs of liquefaction, and consequently that the critical point must be still lower. This would force us to anticipate that the boiling-point of the liquid will be about 5 degrees absolute, or liquid helium will be four times more volatile than liquid hydrogen, just as liquid hydrogen is four times more volatile than liquid air. Although the liquefaction of the gas is a problem for the future, this does not prevent us from safely anticipating some of the properties of the fluid body. It would be twice as dense as liquid hydrogen, with a critical pressure of only 4 or 5 atmospheres. The liquid would possess a very feeble surface-tension, and its compressibility and expansibility would be about four times that of liquid hydrogen, while the heat required to vaporise the molecule would be about one-fourth that of liquid hydrogen. Heating the liquid 1 degree above its boiling-point would raise the pressure by 1½ atmospheres, which is more than four times the increment for liquid hydrogen. The liquid would be only seventeen times denser than its vapour, whereas liquid hydrogen is sixty-five times denser than the gas it gives off. Only some 3 or 4 degrees would separate the critical temperature from the boiling-point and the melting-point, whereas in liquid hydrogen the separation is respectively 10 and 15 degrees. As the liquid refractivities for oxygen, nitrogen and hydrogen are closely proportional to the gaseous values, and as Lord Rayleigh has shown that helium has only one-fourth the refractivity of hydrogen, although it is twice as dense, we must infer that the refractivity of liquid helium would also be about one-fourth that of liquid hydrogen. Now hydrogen has the smallest refractivity of any known liquid, and yet liquid helium will have only about one-fourth of this value—comparable, in fact, with liquid hydrogen just below its critical point. This means that the liquid will be quite exceptional in its optical properties, and very difficult to see. This may be the explanation of why no mist has been seen on its adiabatic expansion from the lowest temperatures. Taking all these remarkable properties of the liquid into consideration, one is afraid to predict that we are at present able to cope with the difficulties involved in its production and collection. Provided the critical point is, however, not below 8 degrees absolute, then from the knowledge of the conditions that are successful in producing a change of state in hydrogen through the use of liquid air, we may safely predict that helium can be liquefied by following similar methods. If, however, the critical point is as low as 6 degrees absolute, then it would be almost hopeless to anticipate success by adopting the process that works so well with hydrogen. The present anticipation is that the gas will succumb after being subjected to this process, only, instead of liquid air under exhaustion being used as the primary cooling agent, liquid hydrogen evaporating under similar circumstances must be employed. In this case, the resulting liquid would require to be collected in a vacuum vessel the outer walls of which are immersed in liquid hydrogen. The practical difficulties and the cost of the operation will be very great; but, on the other hand, the descent to a temperature within 5 degrees of the zero would open out new vistas of scientific inquiry, which would add immensely to our knowledge of the properties of matter. To command in our laboratories a temperature which would be equivalent to that which a comet might reach at an infinite distance from the sun would indeed be a great triumph for science. If the present Royal Institution attack on helium should fail, then we must ultimately succeed by adopting a process based on the mechanical production of cold through the performance of external work. When a turbine can be worked by compressed helium, the whole of the mechanism and circuits being kept surrounded with liquid hydrogen, then we need hardly doubt that the liquefaction will be effected. In



all probability gases other than helium will be discovered of greater volatility than hydrogen. It was at the British Association Meeting in 1896 that I made the first suggestion of the probable existence of an unknown element which would be found to fill up the gap between argon and helium, and this anticipation was soon taken up by others and ultimately confirmed. Later, in the Bakerian Lecture for 1901, I was led to infer that another member of the helium group might exist having the atomic weight about 2, and this would give us a gas still more volatile, with which the absolute zero might be still more nearly approached. It is to be hoped that some such element or elements may yet be isolated and identified as coronium or nebulium. If amongst the unknown gases possessing a very low critical point some have a high critical pressure instead of a low one, which ordinary experience would lead us to anticipate, then such difficultly liquefiable gases would produce fluids having different physical properties from any of those with which we are acquainted. Again, gases may exist having smaller atomic weights and densities than hydrogen, yet all such gases must, according to our present views of the gaseous state, be capable of liquefaction before the zero of temperature is reached. The chemists of the future will find ample scope for investigation within the apparently limited range of temperature which separates solid hydrogen from the zero. Indeed, great as is the sentimental interest attached to the liquefaction of these refractory gases, the importance of the achievement lies rather in the fact that it opens out new fields of research and enormously widens the horizon of physical science, enabling the natural philosopher to study the properties and behaviour of matter under entirely novel conditions. This department of inquiry is as yet only in its infancy, but speedy and extensive developments may be looked for, since within recent years several special cryogenic laboratories have been established for the prosecution of such researches, and a liquid-air plant is becoming a common adjunct to the equipment of the ordinary laboratory.

#### *The Upper Air and Auroras.*

The present liquid ocean, neglecting everything for the moment but the water, was at a previous period of the earth's history part of the atmosphere, and its condensation has been brought about by the gradual cooling of the earth's surface. This resulting ocean is subjected to the pressure of the remaining uncondensed gases, and as these are slightly soluble they dissolve to some extent in the fluid. The gases in solution can be taken out by distillation or by exhausting the water, and if we compare their volume with the volume of water as steam, we should find about 1 volume of air in 60,000 volumes of steam. This would then be about the rough proportion of the relatively permanent gas to condensable gas which existed in the case of the vaporised ocean. Now let us assume the surface of the earth gradually cooled to some 200 degrees below the freezing-point; then, after all the present ocean was frozen, and the climate became three times more intense than any Arctic frost, a new ocean of liquid air would appear, covering the entire surface of the frozen globe about thirty-five feet deep. We may now apply the same reasoning to the liquid air ocean that we formerly did to the water one, and this would lead us to anticipate that it might contain in solution some gases that may be far less condensable than the chief constituents of the fluid. In order to separate them we must imitate the method of taking the gases out of water. Assume a sample of liquid air cooled to the low temperature that can be reached by its own evaporation, connected by a pipe to a condenser cooled in liquid hydrogen; then any volatile gases present in solution will distil over with the first portions of the air, and can be pumped off, being uncondensable at the temperature of the condenser. In this way, a gas mixture, containing, of the known gases, free hydrogen, helium and neon, has been separated from liquid air. It is interesting to note in passing that the relative volatilities of water and oxygen are in the same ratio as those of liquid air and hydrogen, so that the analogy between the ocean of water and that of liquid air has another suggestive parallel. The total uncondensable gas separated in this way amounts to about one fifty-thousandth of the volume of the air, which is about the same proportion as the air dissolved in water. That free hydrogen exists in air in small amount is conclusively proved, but the actual proportion found by the process is very much smaller than Gautier has estimated by the combustion method. The recent experiments of Lord Rayleigh show that Gautier, who estimated the hydrogen present as one five-thousandth,

has in some way produced more hydrogen than he can manage to extract from pure air by a repetition of the same process. The spectroscopic examination of these gases throws new light upon the question of the aurora and the nature of the upper air. On passing electric discharges through the tubes containing the most volatile of the atmospheric gases, they glow with a bright orange light, which is especially marked at the negative pole. The spectroscope shows that this light consists, in the visible part of the spectrum, chiefly of a succession of strong rays in the red, orange and yellow, attributed to hydrogen, helium and neon. Besides these, a vast number of rays, generally less brilliant, are distributed through the whole length of the visible spectrum. The greater part of these rays are of, as yet, unknown origin. The violet and ultra-violet part of the spectrum rivals in strength that of the red and yellow rays. As these gases probably include some of the gases that pervade interplanetary space, search was made for the prominent nebular, coronal and auroral lines. No definite lines agreeing with the nebular spectrum could be found, but many lines occurred closely coincident with the coronal and auroral spectrum. But before discussing the spectroscopic problem it will be necessary to consider the nature and condition of the upper air.

According to the old law of Dalton, supported by the modern dynamical theory of gases, each constituent of the atmosphere while acted upon by the force of gravity forms a separate atmosphere, completely independent, except as to temperature, of the others, and the relations between the common temperature and the pressure and altitude for each specific atmosphere can be definitely expressed. If we assume the altitude and temperature known, then the pressure can be ascertained for the same height in the case of each of the gaseous constituents, and in this way the percentage composition of the atmosphere at that place may be deduced. Suppose we start with a surface atmosphere having the composition of our air, only containing two ten-thousandths of hydrogen, then at thirty-seven miles, if a sample could be procured for analysis, we believe that it would be found to contain 12 per cent. of hydrogen and only 10 per cent. of oxygen. The carbonic acid practically disappears; and by the time we reach forty-seven miles, where the temperature is *minus* 132 degrees, assuming a gradient of 3.2 degrees per mile, the nitrogen and oxygen have so thinned out that the only constituent of the upper air which is left is hydrogen. If the gradient of temperature were doubled, the elimination of the nitrogen and oxygen would take place by the time thirty-seven miles was reached, with a temperature of *minus* 220 degrees. The permanence of the composition of the air at the highest altitudes, as deduced from the basis of the dynamical theory of gases, has been discussed by Stoney, Bryan, and others. It would appear that there is a consensus of opinion that the rate at which gases like hydrogen and helium could escape from the earth's atmosphere would be excessively slow. Considering that to compensate any such loss the same gases are being supplied by actions taking place in the crust of the earth, we may safely regard them as necessarily permanent constituents of the upper air. The temperature at the elevations we have been discussing would not be sufficient to cause any liquefaction of the nitrogen and oxygen, the pressure being so low. If we assume the mean temperature as about the boiling-point of oxygen at atmospheric pressure, then a considerable amount of the carbonic acid must solidify as a mist, if the air from a lower level be cooled to this temperature; and the same result might take place with other gases of relatively small volatility which occur in air. This would explain the clouds that have been seen at an elevation of fifty miles, without assuming the possibility of water vapour being carried up so high. The temperature of the upper air must be above that on the vapour pressure curve corresponding to the barometric pressure at the locality, otherwise liquid condensation must take place. In other words, the temperature must be above the dew-point of air at that place. At higher elevations, on any reasonable assumption of temperature distribution, we inevitably reach a temperature where the air would condense, just as Fourier and Poisson supposed it would, unless the temperature is arrested in some way from approaching the zero. Both ultra-violet absorption and the prevalence of electric storms may have something to do with the maintenance of a higher mean temperature. The whole mass of the air above forty miles is not more than one seven-hundredth part of the total mass of the atmosphere, so that any rain or snow of liquid

or solid air, if it did occur, would necessarily be of a very tenuous description. In any case, the dense gases tend to accumulate in the lower strata, and the lighter ones to predominate at the higher altitudes, always assuming that a steady state of equilibrium has been reached. It must be observed, however, that a sample of air taken at an elevation of nine miles has shown no difference in composition from that at the ground, whereas, according to our hypothesis, the oxygen ought to have been diminished to 17 per cent., and the carbonic acid should also have become much less. This can only be explained by assuming that a large intermixture of different layers of the atmosphere is still taking place at this elevation. This is confirmed by a study of the motions of clouds about six miles high, which reveals an average velocity of the air currents of some seventy miles an hour; such violent winds must be the means of causing the intermingling of different atmospheric strata. Some clouds, however, during hot and thundery weather, have been seen to reach an elevation of seventeen miles, so that we have direct proof that on occasion the lower layers of atmosphere are carried to a great elevation. The existence of an atmosphere at more than a hundred miles above the surface of the earth is revealed to us by the appearance of meteors and fireballs, and when we can take photographs of the spectrum of such apparitions we shall learn a great deal about the composition of the upper air. In the meantime Pickering's solitary spectrum of a meteor reveals an atmosphere of hydrogen and helium, and so far this is corroborative of the doctrine we have been discussing. It has long been recognised that the aurora is the result of electric discharges within the limits of the earth's atmosphere, but it was difficult to understand why its spectrum should be so entirely different from anything which could be produced artificially by electric discharges through rarefied air at the surface of the earth. Writing in 1879, Rand Capron, after collecting all the recorded observations, was able to enumerate no more than nine auroral rays, of which but one could with any probability be identified with rays emitted by atmospheric air under an electric discharge. Vogel attributed this want of agreement between nature and experiment, in a vague way, to difference of temperature and pressure; and Zollner thought the auroral spectrum to be one of a different order, in the sense in which the line and band spectra of nitrogen are said to be of different orders. Such statements were merely confessions of ignorance. But since that time observations of the spectra of auroras have been greatly multiplied, chiefly through the Swedish and Danish Polar Expeditions, and the length of spectrum recorded on the ultra-violet side has been greatly extended by the use of photography, so that, in a recent discussion of the results, M. Henri Stassano is able to enumerate upwards of one hundred auroral rays, of which the wave-length is more or less approximately known, some of them far in the ultra-violet. Of this large number of rays he is able to identify, within the probable limits of errors of observation, about two-thirds as rays, which Prof. Liveing and myself have observed to be emitted by the most volatile gases of atmospheric air unliquefiable at the temperature of liquid hydrogen. Most of the remainder he ascribes to argon, and some he might, with more probability, have identified with krypton or xenon rays, if he had been aware of the publication of wave-lengths of the spectra of those gases, and the identification of one of the highest rays of krypton with that most characteristic of auroras. The rosy tint often seen in auroras, particularly in the streamers, appears to be due mainly to neon, of which the spectrum is remarkably rich in red and orange rays. One or two neon rays are amongst those most frequently observed, while the red ray of hydrogen and one red ray of krypton have been noticed only once. The predominance of neon is not surprising, seeing that from its relatively greater proportion in air and its low density it must tend to concentrate at higher elevations. So large a number of probable identifications warrants the belief that we may yet be able to reproduce in our laboratories the auroral spectrum in its entirety. It is true that we have still to account for the appearance of some, and the absence of other, rays of the newly discovered gases, which in the way in which we stimulate them appear to be equally brilliant, and for the absence, with one doubtful exception, of all the rays of nitrogen. If we cannot give the reason of this, it is because we do not know the mechanism of luminescence—nor even whether the particles which carry the electricity are themselves luminous, or whether they only produce stresses causing other particles which

encounter them to vibrate; yet we are certain that an electric discharge in a highly rarefied mixture of gases lights one element and not another, in a way which, to our ignorance, seems capricious. The Swedish North Polar Expedition concluded from a great number of trigonometrical measurements that the average above the ground of the base of the aurora was fifty kilometres (thirty-four miles) at Cape Thorsden, Spitzbergen; at this height the pressure of the nitrogen of the atmosphere would be only about one-tenth of a millimetre, and Moissan and Deslandres have found that in atmospheric air at pressures less than one millimetre the rays of nitrogen and oxygen fade and are replaced by those of argon and by five new rays which Stassano identifies with rays of the more volatile gases measured by us. Also Collie and Ramsay's observations on the distance to which electrical discharges of equal potential traverse different gases explosively throw much light on the question; for they find that, while for helium and neon this distance is from 250 to 300 mm., for argon it is 45½ mm., for hydrogen it is 39 mm., and for air and oxygen still less. This indicates that a good deal depends on the very constitution of the gases themselves, and certainly helps us to understand why neon and argon, which exist in the atmosphere in larger proportions than helium, krypton or xenon, should make their appearance in the spectrum of auroras almost to the exclusion of nitrogen and oxygen. How much depends, not only on the constitution and it may be temperature of the gases, but also on the character of the electric discharge, is evident from the difference between the spectra at the cathode and anode in different gases, notably in nitrogen and argon, and not less remarkably in the more volatile compounds of the atmosphere. Paulsen thinks the auroral spectrum wholly due to cathodic rays. Without stopping to discuss that question, it is certain that changes in the character of the electric discharge produce definite changes in the spectra excited by them. It has long been known that in many spectra the rays which are inconspicuous with an uncondensed electric discharge become very pronounced when a Leyden jar is in the circuit. This used to be ascribed to a higher temperature in this condensed spark, though measurements of that temperature have not borne out the explanation. Schuster and Hemsalech have shown that these changes of spectra are in part due to the oscillatory character of the condenser discharge which may be enhanced by self-induction, and the corresponding change of spectrum thereby made more pronounced. Lightning we should expect to resemble condensed discharge much more than aurora, but this is not borne out by the spectrum. Pickering's recent analysis of the spectrum of a flash obtained by photography shows, out of nineteen lines measured by him, only two which can be assigned with probability to nitrogen and oxygen, while three hydrogen rays most likely due to water are very conspicuous, and eleven may be reasonably ascribed to argon, krypton and xenon, one to more volatile gas of the neon class, and the brightest ray of all is but a very little less refrangible than the characteristic auroral ray, and coincides with a strong ray of calcium, but also lies between, and close to, an argon and a neon ray, neither of them weak rays. There may be some doubt about the identification of the spectral rays of auroras because of the wide limits of the probable errors in measuring wave-lengths so faint as most of them are, but there is no such doubt about the wave-lengths of the rays in solar protuberances measured by Deslandres and Hale. Stassano found that these rays, forty-four in number, lying between the Fraunhofer line F and 3148 in the ultra-violet, agree very closely with rays which Prof. Liveing and myself measured in the spectra of the most volatile atmospheric gases. It will be remembered that one of the earliest suggestions as to the nature of solar prominences was that they were solar auroras. This supposition helped to explain the marvellous rapidity of their changes, and the apparent suspension of brilliant self-luminous clouds at enormous heights above the sun's surface. Now the identification of the rays of their spectra with those of the most volatile gases, which also furnish many of the auroral rays, certainly supports that suggestion. A stronger support, however, seems to be given to it by the results obtained at the total eclipse of May, 1901, by the American expedition to Sumatra. In the *Astrophysical Journal* for June last is a list of 339 lines in the spectrum of the corona photographed by Humphreys, during totality, with a very large concave grating. Of these no fewer than 209 do not differ from lines we have measured in the most volatile gases of the atmo-



sphere, or in krypton or xenon, by more than one unit of wave-length on Ångström's scale, a quantity within the limit of probable error. Of the remainder, a good many agree to a like degree with argon lines, a very few with oxygen lines and still fewer with nitrogen lines; the characteristic green auroral ray, which is not in the range of Humphreys' photographs, also agrees within a small fraction of a unit of wave-length with one of the rays emitted by the most volatile atmospheric gas. Taking into account the Fraunhofer lines H, K and G, usually ascribed to calcium, there remain only fifty-five lines of the 339 unaccounted for to the degree of probability indicated. Of these considerably more than half are very weak lines which have not depicted themselves on more than one of the six films exposed, and extend but a very short distance into the sun's atmosphere. There are, however, seven which are stronger lines, and reach to a considerable height above the sun's rim, and all have depicted themselves on at least four of the six films. If there be no considerable error in the wave-lengths assigned (and such is not likely to be the case), these lines may perhaps be due to some volatile element which may yet be discovered in our atmosphere. However that may be, the very great number of close coincidences between the auroral rays and those which are emitted under electric excitement by gases of our atmosphere almost constrains us to believe, what is indeed most probable on other grounds, that the sun's coronal atmosphere is composed of the same substances as the earth's, and that it is rendered luminous in the same way—namely, by electric discharges. This conclusion has plainly an important bearing on the explanation which should be given of the outburst of new stars and of the extraordinary and rapid changes in their spectra. Moreover, leaving on one side the question whether gases ever become luminous by the direct action of heat, apart from such transfers of energy as occur in chemical change and electric disturbance, it demands a revision of the theories which attribute more permanent differences between the spectra of different stars to differences of temperature, and a fuller consideration of the question whether they cannot with better reason be explained by differences in the electric conditions which prevail in the stellar atmosphere.

If we turn to the question what is the cause of the electric discharges which are generally believed to occasion auroras, but of which little more has hitherto been known than that they are connected with sun-spots and solar eruptions, recent studies of electric discharges in high vacua, with which the names of Crookes, Röntgen, Lenard, and J. J. Thomson will always be associated, have opened the way for Arrhenius to suggest a definite and rational answer. He points out that the frequent disturbances which we know to occur in the sun must cause electric discharges in the sun's atmosphere far exceeding any that occur in that of the earth. These will be attended with an ionisation of the gases, and the negative ions will stream away through the outer atmosphere of the sun into the interplanetary space, becoming, as Wilson has shown, nuclei of aggregation of condensable vapours and cosmic dust. The liquid and solid particles thus formed will be of various sizes; the larger will gravitate back to the sun, while those with diameters less than one and a half thousandths of a millimetre, but nevertheless greater than a wave-length of light, will, in accordance with Clerk-Maxwell's electromagnetic theory, be driven away from the sun by the incidence of the solar rays upon them, with velocities which may become enormous, until they meet other celestial bodies, or increase their dimensions by picking up more cosmic dust or diminish them by evaporation. The earth will catch its share of such particles on the side which is turned towards the sun, and its upper atmosphere will thereby become negatively electrified until the potential of the charge reaches such a point that a discharge occurs, which will be repeated as more charged particles reach the earth. This theory not only accounts for the auroral discharges, and the coincidence of their times of greatest frequency with those of the maxima of sunspots, but also for the minor maxima and minima. The vernal and autumnal maxima occur when the line through the earth and sun has its greatest inclination to the solar equator, so that the earth is more directly exposed to the region of maximum of sunspots, while the twenty-six days period corresponds closely with the period of rotation of that part of the solar surface where faculæ are most abundant. J. J. Thomson has pointed out, as a consequence of the Richardson observations, that negative ions will be constantly

streaming from the sun merely regarded as a hot body, but this is not inconsistent with the supposition that there will be an excess of this emission in eruptions, and from the regions of faculæ. Arrhenius' theory accounts also, in a way which seems the most satisfactory hitherto enunciated, for the appearances presented by comets. The solid parts of these objects absorb the sun's rays, and as they approach the sun become heated on the side turned towards him until the volatile substances frozen in or upon them are evaporated and diffused in the gaseous state in surrounding space, where they get cooled to the temperature of liquefaction and aggregated in drops about the negative ions. The larger of these drops gravitate towards the sun and form clouds of the coma about the head, while the smaller are driven by the incidence of the sun's light upon them away from the sun and form the tail. The curvature of the tail depends, as Bredichin has shown, on the rate at which the particles are driven, which in turn depends on the size and specific gravity of the particles, and these will vary with the density of the vapour from which they are formed and the frequency of the negative ions which collect them. In any case Arrhenius' theory is a most suggestive one, not only with reference to auroras and comets, and the solar corona and chromosphere, but also as to the constitution of the photosphere itself.

#### *Various Low-Temperature Researches.*

We may now summarise some of the results which have already been attained by low-temperature studies. In the first place, the great majority of chemical interactions are entirely suspended, but an element of such exceptional powers of combination as fluorine is still active at the temperature of liquid air. Whether solid fluorine and liquid hydrogen would interact no one can at present say. Bodies naturally become denser, but even a highly expansive substance like ice does not appear to reach the density of water at the lowest temperature. This is confirmatory of the view that the particles of matter under such conditions are not packed in the closest possible way. The force of cohesion is greatly increased at low temperatures, as is shown by the additional stress required to rupture metallic wires. This fact is of interest in connection with two conflicting theories of matter. Lord Kelvin's view is that the forces that hold together the particles of bodies may be accounted for without assuming any other agency than gravitation or any other law than the Newtonian. An opposite view is that the phenomena of the aggregation of molecules depend upon the molecular vibration as a physical cause. Hence, at the zero of absolute temperature, this vibrating energy being in complete abeyance, the phenomena of cohesion should cease to exist, and matter generally be reduced to an incoherent heap of cosmic dust. This second view receives no support from experiment.

The photographic action of light is diminished at the temperature of liquid air to about 20 per cent. of its ordinary efficiency, and at the still lower temperature of liquid hydrogen only about 10 per cent. of the original sensitivity remains. At the temperature of liquid air or liquid hydrogen a large range of organic bodies and many inorganic ones acquire under exposure to violet light the property of phosphorescence. Such bodies glow faintly so long as they are kept cold, but become exceedingly brilliant during the period when the temperature is rising. Even solid air is a phosphorescent body. All the alkaline earth sulphides which phosphoresce brilliantly at the ordinary temperature lose this property when cooled, to be revived on heating; but such bodies in the first instance may be stimulated through the absorption of light at the lowest temperatures. Radio-active bodies, on the other hand, like radium, which are naturally self-luminous, maintain this luminosity unimpaired at the very lowest temperatures, and are still capable of inducing phosphorescence in bodies like the platino-cyanides. Some crystals become for a time self-luminous when cooled in liquid air or hydrogen, owing to the induced electric stimulation causing discharges between the crystal molecules. This phenomenon is very pronounced with nitrate of uranium and some platino-cyanides.

In conjunction with Prof. Fleming a long series of experiments was made on the electric and magnetic properties of bodies at low temperatures. The subjects that have been under investigation may be classified as follows: The Thermo-Electric Powers of Pure Metals; the Magnetic Properties of Iron and Steel; Dielectric Constants; the Magnetic and Electric Constants of Liquid Oxygen; Magnetic Susceptibility.

The investigations have shown that electric conductivity in pure metals varies almost inversely as the absolute temperature down to *minus* 200 degrees, but that this law is greatly affected by the presence of the most minute amount of impurity. Hence the results amount to a proof that electric resistance in pure metals is closely dependent upon the molecular or atomic motion which gives rise to temperature, and that the process by which the energy constituting what is called an electric current is dissipated essentially depends upon non-homogeneity of structure and upon the absolute temperature of the material. It might be inferred that at the zero of absolute temperature resistance would vanish altogether, and all pure metals become perfect conductors of electricity. This conclusion, however, has been rendered very doubtful by subsequent observations made at still lower temperatures, which appear to point to an ultimate finite resistance. Thus the temperature at which copper was assumed to have no resistance was *minus* 223 degrees, but that metal has been cooled to *minus* 253 degrees without getting rid of all resistance. The reduction in resistance of some of the metals at the boiling-point of hydrogen is very remarkable. Thus copper has only 1 per cent., gold and platinum 3 per cent., and silver 4 per cent. of the resistance they possessed at zero C., but iron still retains 12 per cent. of its initial resistance. In the case of alloys and impure metals, cold brings about a much smaller decrease in resistivity, and in the case of carbon and insulators like gutta-percha, glass, ebonite, &c., their resistivity steadily increases. The enormous increase in resistance of bismuth when transversely magnetised and cooled was also discovered in the course of these experiments. The study of dielectric constants at low temperatures has resulted in the discovery of some interesting facts. A fundamental deduction from Maxwell's theory is that the square of the refractive index of a body should be the same number as its dielectric constant. So far, however, from this being the case generally, the exceptions are far more numerous than the coincidences. It has been shown in the case of many substances, such as ice and glass, that an increase in the frequency of the alternating electromotive force results in a reduction of the dielectric constant to a value more consistent with Maxwell's law. By experiments upon many substances it is shown that even a moderate increase of frequency brings the large dielectric constant to values quite near to that required by Maxwell's law. It was thus shown that low temperature has the same effect as high frequency in annulling the abnormal dielectric values. The exact measurement of the dielectric constant of liquid oxygen as well as its magnetic permeability, combined with the optical determination of the refractive index, showed that liquid oxygen strictly obeys Maxwell's electro-optic law even at very low electric frequencies. In magnetic work the result of greatest value is the proof that magnetic susceptibility varies inversely as the absolute temperature. This shows that the magnetisation of paramagnetic bodies is an affair of orientation of molecules, and it suggests that at the absolute zero all the feebly paramagnetic bodies will be strongly magnetic. The diamagnetism of bismuth was found to be increased at low temperatures. The magnetic moment of a steel magnet is temporarily increased by cooling in liquid air, but the increase seems to have reached a limit, because on further cooling to the temperature of liquid hydrogen hardly any further change was observed. The study of the thermo-electric relations of the metals at low temperatures resulted in a great extension of the well-known Tait Thermo-Electric Diagram. Tait found that the thermo-electric power of the metals could be expressed by a linear function of the absolute temperature, but at the extreme range of temperature now under consideration this law was found not to hold generally; and further, it appeared that many abrupt electric changes take place, which originate probably from specific molecular changes occurring in the metal. The thermo-electric neutral points of certain metals, such as lead and gold, which are located about or below the boiling-point of hydrogen, have been found to be a convenient means of defining specific temperatures in this exceptional part of the scale.

The effect of cold upon the life of living organisms is a matter of great intrinsic interest, as well as of wide theoretical importance. Experiment indicates that moderately high temperatures are much more fatal, at least to the lower forms of life, than are exceedingly low ones. Prof. McKendrick froze for an hour at a temperature of 182° C. samples of meat, milk, &c., in

sealed tubes; when these were opened after being kept at blood heat for a few days, their contents were found to be quite putrid. More recently some more elaborate tests were carried out at the Jenner Institute of Preventive Medicine on a series of typical bacteria. These were exposed to the temperature of liquid air for twenty hours, but their vitality was not affected, their functional activities remained unimpaired, and the cultures which they yielded were normal in every respect. The same result was obtained when liquid hydrogen was substituted for air. A similar persistence of life in seeds has been demonstrated even at the lowest temperatures; they were frozen for over a hundred hours in liquid air, at the instance of Messrs. Brown and Escombe, with no other result than to affect their protoplasm with a certain inertness, from which it recovered with warmth. Subsequently commercial samples of barley, pea, vegetable-marrow and mustard seeds were literally steeped for six hours in liquid hydrogen at the Royal Institution, yet when they were sown by Sir W. T. Thiselton-Dyer at Kew in the ordinary way, the proportion in which germination occurred was no less than in the other batches of the same seeds which had suffered no abnormal treatment. Bacteria are minute vegetable cells, the standard of measurement for which is the "mikron." Yet it has been found possible to completely triturate these microscopic cells, when the operation is carried out at the temperature of liquid air, the cells then being frozen into hard, breakable masses. The typhoid organism has been treated in this way, and the cell plasma obtained for the purpose of studying its toxic and immunising properties. It would hardly have been anticipated that liquid air should find such immediate application in biological research. A research by Prof. Macfadyen, just concluded, has shown that many varieties of micro-organisms can be exposed to the temperature of liquid air for a period of six months without any appreciable loss of vitality, although at such a temperature the ordinary chemical processes of the cell must cease. At such a temperature the cells cannot be said to be either alive or dead, in the ordinary acceptance of these words. It is a new and hitherto unobtainable condition of living matter—a third state. A final instance of the application of the above methods may be given. Certain species of bacteria during the course of their vital processes are capable of emitting light. If, however, the cells be broken up at the temperature of liquid air, and the crushed contents brought to the ordinary temperature, the luminosity function is found to have disappeared. This points to the luminosity not being due to the action of a ferment—a "Luciferase"—but as being essentially bound up with the vital processes of the cells, and dependent for its production on the intact organisation of the cell. These attempts to study by frigorific methods the physiology of the cell have already yielded valuable and encouraging results, and it is to be hoped that this line of investigation will continue to be vigorously prosecuted at the Jenner Institute.

And now, to conclude an address which must have sorely taxed your patience, I may remind you that I commenced by referring to the plaint of Elizabethan science, that cold was not a natural available product. In the course of a long struggle with nature, man, by the application of intelligent and steady industry, has acquired a control over this agency which enables him to produce it at will, and with almost any degree of intensity, short of a limit defined by the very nature of things. But the success in working what appears, at first sight, to be a quarry of research that would soon suffer exhaustion, has only brought him to the threshold of new labyrinths, the entanglements of which frustrate, with a seemingly invulnerable complexity, the hopes of further progress. In a legitimate sense all genuine scientific workers feel that they are "the inheritors of unfulfilled renown." The battlefields of science are the centres of a perpetual warfare, in which there is no hope of final victory, although partial conquest is ever triumphantly encouraging the continuance of the disciplined and strenuous attack on the seemingly impregnable fortress of Nature. To serve in the scientific army, to have shown some initiative, and to be rewarded by the consciousness that in the eyes of his comrades he bears the accredited accolade of successful endeavour, is enough to satisfy the legitimate ambition of every earnest student of Nature. The real warranty that the march of progress in the future will be as glorious as in the past lies in the perpetual reinforcement of the scientific ranks by recruits animated by such a spirit, and proud to obtain such a reward.



## SECTION A.

## MATHEMATICS AND PHYSICS.

OPENING ADDRESS BY PROF. JOHN PURSER, M.A., LL.D.,  
M.R.I.A., PRESIDENT OF THE SECTION.

IN opening our proceedings to-day allow me at the outset to express my deep sense of the honour the Association has conferred upon me in asking me to preside over this Section.

My predecessors in this Chair have usually given you a survey of some department of Mathematics or Physics, tracing what had been already accomplished in that department and indicating the nature of the problems which still awaited solution.

May I crave your indulgence if I deviate from this course and, following the suggestion of some of my friends, take the opportunity of the Association meeting on Irish soil to give you a slight historical sketch of our Irish School of Mathematics and Physics?

In attempting such a review, for the sake of brevity as well as for other reasons, I shall confine it to the work of those who are no longer with us, and I would not carry it further back than the beginning of last century. This seems a natural starting point, as there was at that time a very marked revival of the study of science in the University of Dublin, a revival largely due to the influence of Provost Bartholomew Lloyd.

Lloyd won his Fellowship in Trinity College a few years before the century opened, and subsequently filled in succession the Chairs of Mathematics and Natural Philosophy. In both departments he imported a radical change into the methods of teaching. By his treatises on Analytical Geometry and on Mechanical Philosophy he introduced the study of what was then called the French Mathematics, in other words the more advanced Analytic Methods, which were in use on the Continent. In 1831 he was appointed Provost of the College, and his tenure of the office, though brief, was signalised by many important improvements and new developments effected in the University teaching.

Dr. Bartholomew Lloyd was President of one of the earliest Meetings of this Association, that held in Dublin in 1835.

His son, Dr. Humphrey Lloyd, had a course which was a singularly close parallel to his father's.

He won his Fellowship in 1824, and succeeded his father in the Chair of Natural Philosophy. He also was afterwards appointed Provost, and he too presided over another Dublin Meeting of this Association, that held in 1857. He also, in this again following in his father's steps, wrote important works on different branches of Physics; "Light and Vision," a systematic treatise on plane as distinct from physical optics, "Lectures on the Wave Theory of Light," and lastly a treatise on "Magnetism."

It is, perhaps, in connection with this latter subject that his most important work was done. He made in association with Sabine an elaborate series of observations on terrestrial magnetism in twenty-four stations in various parts of Ireland, and when subsequently, at the instance of your Association and of the Royal Society, the Government established magnetic observatories in different parts of the world, it was Lloyd who was entrusted with the task of drawing up the manual of instructions for the observers and of receiving their reports.

In the interval between the two Lloyds another name claims attention. Dr. Romney Robinson occupied during an exceptionally long life a much honoured and influential position amongst men of science. It was in this city he received his early education, for when young Robinson was only nine years of age his father had occasion to move to Belfast, and he placed his son under Dr. Bruce, a well-known schoolmaster of those days. Robinson was afterwards sent to Trinity College, and after a distinguished course was elected to a Fellowship in 1814. For some years he lectured in college as Deputy Professor of Natural Philosophy. He relinquished his Fellowship on obtaining a College living, and a few years later was appointed Astronomer in charge of the Armagh Observatory. The results of his observations were considered so valuable as to be used by the German astronomer Argeländer in determining the proper motions of stars. The range, however, of his published papers was by no means confined to Astronomy, but extended to the most varied subjects, Heat, Electricity, Magnetism, Turbines, Air-pumps, Fog-signals, and others. He is best known to the general public as the inventor of the Cup Anemometer. He was chosen to preside over the Birmingham Meeting of this Association in 1849.

Robinson was intimately associated with Lord Rosse and keenly interested in the experiments which culminated in the construction of the great reflector in Parsonstown. This naturally leads us to speak of Lord Rosse himself. Few scientific achievements took a greater hold upon the public mind than the successful completion of his great telescope. Only those who have read in Lord Rosse's own papers the description of the many difficulties he had to contend with in forging and polishing that wonderful speculum, harder than steel yet more brittle than glass, can adequately appreciate the patience and resource with which those difficulties were successfully overcome.

Of the results obtained with this instrument the most notable were in the observation of the Nebulæ, a department where its unsurpassed power of light-concentration came fully into play. No doubt at the time public attention was most excited by the resolution of a number of hitherto supposed nebulæ into star clusters, leading to the premature conclusion in the minds of those less instructed that all the nebulæ might ultimately be so resolved. To us, however, a far greater interest attaches to the observation of the structure of what we now know to be genuine nebulæ, especially the great discovery that these had in many cases a peculiar spiral form. All previous telescopes had failed to detect this spiral character; but the drawings taken by Lord Rosse and his assistants put this feature beyond question, and these have been fully confirmed in recent years, when more accurate delineations were obtained by photography. I need not dwell upon the significance of this form, indicating, as it does, a rotatory movement in these mighty masses and lying in with, if not actually confirming, Laplace's Nebular Hypothesis.

Sir William Rowan Hamilton was undoubtedly the most striking figure in the annals of the Dublin School of Mathematics. *In limine* we must make good our right to call him an Irishman, for his greatest admirer and disciple, Prof. Tait, has claimed him for a countryman of his own, asserting that Hamilton's grandfather was a Scotchman who migrated to Dublin with his two young sons. That this was a complete misconception has been abundantly proved by the careful investigations of his friend and biographer, Dr. R. P. Graves, who shows conclusively that the only known strain of Scotch blood in Hamilton came through his grandmother, who was the daughter of a minister of the Scottish Kirk.

It is interesting to find how early Hamilton's remarkable mental powers began to show themselves. Dr. Graves has given us a letter from his mother in which she writes to her sister of the marvellous precocity of her little four-year-old boy, telling how "he reads Latin, Greek, and Hebrew."

His mental development did not belie these early indications, for at the age of thirteen, thanks to the teaching and care of his uncle, who was a most extraordinary linguist, he had not only acquired a considerable knowledge of the classics and the modern European languages, but also attained some proficiency in Arabic, Sanscrit and Persian. His mathematical studies, on the other hand, appear to have been carried on without help from anyone, and it is noteworthy that he does not seem to have used common text-books, but to have gone direct to the great original authors; *e.g.*, he read his algebra in Newton's "Arithmetica Universalis"; while at the age of fifteen he set himself to read the "Principia," and two years later began a systematic study of Laplace's "Mécanique Céleste." His own estimate of his powers may be gathered from a characteristic letter to his sister written just after he had entered Trinity College:—

"One thing only have I to regret in the direction of my studies, that they should be diverted—or rather rudely forced—by the College course from their natural bent and favourite channel. That bent, you know, is science—science in its most exalted heights, in its most secret recesses. It has so captivated me, so seized on, I may say, my affections that my attention to classical studies is an effort and an irksome one; and I own that, before I entered College, I did not hope that in them I would rise above mediocrity. My success surprised me, but it has also given me a spur by holding out a prospect that even in the less agreeable part of my business I may hope still to succeed."

This letter is interesting as indicating on Hamilton's part a consciousness wherein lay his real strength and vocation. Not that his interest in literature ever abated. To the last he loved to try his hand at poetical composition, frequently inserting in his letters to his friends sonnets of his own.

He knew Wordsworth intimately, and the poet, to whom he sent some of his productions, gives him the following candid advice :—

"It would be insincere not to say that something of a style more terse and a harmony more accurately balanced must be acquired before the bodily form of your verses will be quite worthy of their living souls. You are perfectly aware of this, though perhaps not in an equal degree with myself; nor is it desirable you should be, for it might tempt you to labour which would divert you from subjects of infinitely greater importance."

Hamilton was first in his College classes in every subject and at every examination, and it was fully expected that he would carry off both the medals in Mathematics and Classics at his Degree when the following circumstances suddenly changed all his plans. Dr. Brinkley, the Professor of Astronomy in the University, was appointed to a Bishopric, and Hamilton, though still an undergraduate, was invited to offer himself for the vacant Chair. Sir George Airy and more than one of the Fellows of Trinity were also candidates, but Hamilton was unanimously elected.

His career as an original author dates from this time, for immediately after his appointment he communicated to the Royal Irish Academy the first of three remarkable papers on "Systems of Rays."

Two striking features may be observed in these papers, as indeed in all his scientific memoirs: the generality and comprehensiveness with which he states his object at the outset and the confidence with which he follows the bold and original lines of treatment which he lays down for himself, and closely connected with this, the determination not to be baffled by any laboriousness of calculations which the application of his method may involve him in. In his first paper he begins by examining what happens to a system of rays of light emanating from a point and subjected to any number of reflections at curved surfaces. He establishes the theorem that such a system will be cut orthogonally by a system of surfaces, the length of the path measured from the original source to any of these surfaces being the same for all the rays. The proof he gives of this theorem is so simple that it now seems almost axiomatic; but it is curious that Malus, who had made the laws of Light his special study, though he suspected that the theorem ought to hold, yet found himself unable to establish it.

Hamilton, now considering the length of the path to any point as a function of the coordinates of that point, and denoting this function by  $V$ , proves that  $V$  satisfies a simple partial differential equation of the first order and proceeds to show the important part the function  $V$  plays in the theory.

He goes on to prove generally that if we are dealing, not with right lines, that is, with paths, for which as between any two points  $\int ds$  is a minimum, but with curved paths

for which  $\int \mu ds$  is a minimum (where  $\mu$  is a function of the coordinates), and a system of such paths be drawn through a given point,  $O$ , the system of surfaces  $V = \text{const.}$  will still cut all the paths at right angles. If we adopt the emission theory of Light, and we take for  $\mu$  the velocity of Light,  $V$  becomes "the Action," and the minimum property which the paths satisfy is the principle of "Least Action." If, on the other hand, we adopt the undulatory theory, and we take for  $\mu$  the reciprocal of the velocity, the minimum property becomes the principle of "Least Time." Thus Hamilton shows that, by altering the significance of  $\mu$ , his method applies to either theory.

Introducing the further conception that  $\mu$  depends, not only on the coordinates of the point, but also on the direction-angles of the ray, he is able to apply his reasoning to rays passing through a crystal. He gives by his method a new and interesting proof of the equation of Fresnel's wave-surface, and arrives at the conclusion, hitherto unnoticed by mathematicians, that this wave-surface possesses four conical cusps and also four special tangent planes, each of which touches the surface, not in one point only, but in an infinite system of points lying in a circle. The physical significance of these theorems is what is known as Conical Refraction.

Having drawn this inference from his mathematical analysis, Hamilton wrote to his friend Dr. Lloyd and asked him to verify it by actual observation, and accordingly Hamilton's paper in the *Transactions* of the Academy is accompanied by another

from Lloyd, describing the beautiful arrangements by which he had succeeded in verifying this remarkable phenomenon in both its varieties.

This striking instance of scientific prediction naturally made a great sensation at the time, appealing, as it did, to a much larger public than the few select mathematicians who were capable of mastering the elaborate treatise on "Systems of Rays."

The experimental skill that was required to obtain these results may be realised from the circumstance that as I have been told the French physicists found themselves unable to repeat the experiment till Lloyd himself went over to Paris with his instruments and showed them the way.

Hamilton was so well satisfied with the success of his new method in dealing with the problems presented by the propagation of Light that full of enthusiasm he proceeded to apply a generalised form of the same method in the investigations of the motion of any material system, and a paper of his was read before the Royal Society in 1834 with the following title: "On a general method in Dynamics by which the Study of the Motions of all free systems of attracting or repelling points is reduced to the Search and Differentiation of one Central Relation, or Characteristic Function."

To show the importance attached by the most competent judges to Hamilton's work in this field of Theoretical Dynamics, we cannot do better than quote the words of his great German contemporary Jacobi, who afterwards himself added to the new theory such valuable developments.

Jacobi writes as follows:—"If a free system of material points is acted on by no other forces than such as arise from their mutual attraction or repulsion, the differential equations of their motion can be represented in a simple manner by means of the partial differential coefficients of a single function of the co-ordinates. Lagrange, who first made this important observation, at the same time showed that this form of the differential equations possesses great importance for Analytical Mechanics. The marked attention, therefore, of mathematicians could not fail to be aroused when Herr Hamilton, Professor of Astronomy in Dublin, indicated in the *Philosophical Transactions* that in the Mechanical problem referred to all the integral equations of motion might be represented in just as simple a manner by means of the Partial Differential Coefficients of a single function. This is undoubtedly the most considerable extension which Analytical Mechanics has received since Lagrange."

It will be of interest to the Section to recall the fact that Hamilton and Jacobi met each other for the first and I fancy the only time at a meeting of this Association, held in Manchester in 1842, at which meeting Jacobi, addressing this Section, called Hamilton "le Lagrange de votre pays."

The last third of Hamilton's life was mainly devoted to the development of his Quaternion Calculus. As early as 1828 his Class Fellow, J. T. Graves, who had been working at the theory of the use of imaginary quantities in Mathematics, wrote an essay on Imaginary Logarithms which he wished to get printed by the Royal Society. There appears to have been some hesitation amongst the leading mathematicians in the Society, notably, Herschel and Peacock, about publishing Graves' paper, as they felt dubious about the accuracy of his reasoning. Hamilton heard of this and wrote earnestly to Herschel defending his friend's conclusions, and it seems as if his generous desire to help his friend first set his own mind working in this direction.

For years his busy brain in the midst of all his other work kept pondering over this question of the interpretation of the imaginary, and he has left us in his "Lectures on Quaternions" an elaborate account of the many systems he devised.

It was only in 1843, fifteen years later, that he first invented the celebrated laws of combination of the quadrantal versors of the Quaternion Calculus. Argand, Cauchy, and others had proposed for space of two dimensions the theory now known as that of the Complex Variable. For them  $x + iy$  meant the vector to the point  $xy$ , and the product of two vectors meant a new vector of the same form, the only law required being that  $i$  operating upon  $i$  was always equivalent to  $-1$ .

Many attempts had been made to form on similar lines a Calculus which should apply to space of three dimensions; but so far all such attempts had proved unsuccessful, the laws by which the new symbols acted upon one another leading to results hopelessly involved. It was here that Hamilton's wonderful faculty of scientific imagination came into play. He proposed that



vector should be denoted by  $ix + jy + kz$ . As in the theory of the complex variable in two dimensions the result of any number of successive operations always preserved the fundamental type  $a + ib$ , so it was desirable that the result of the successive operations of his vectors should issue in an equally simple fundamental type. This end he found he could attain if he discarded the commutative principle which hitherto had barred his own progress and that of others, yet preserving the distributive and associative principles, and finally one happy evening he arrived at the beautifully simple laws by which the symbols of this Calculus act upon each other; that not only  $i^2 = j^2 = k^2 = -1$ , but also that  $ij = -ji = k$ ,  $jk = -kj = i$ ,  $ki = -ik = j$ .

Though it was thus—as the product, that is, of two vectors—that the Quaternion first presented itself to Hamilton, he of course saw that it immediately followed that it might be regarded as the ratio of two vectors, in other words the operation which turned one vector into another. In fact in the more synthetic exposition which is contained in “The Elements” he makes this latter the starting definition of the Quaternion.

It is noteworthy that this, the more complete and systematic presentation of the subject by its illustrious author, may be said to owe its origin to the keen interest my predecessor, Prof. Tait, took in the new Calculus, of which, as you know, he ever afterwards remained the most ardent champion. This interest led him to seek from Dr. Andrews an introduction to Hamilton, and the encouragement came to Hamilton at an opportune moment, for he wrote:—

“It was useful to me to have my attention recalled to the whole subject of the Quaternions, which I had been almost trying to forget, partly under the impression that nobody cared or would soon care about them. The result seems likely to be that I shall go on to write some such ‘Manual,’ but necessarily a very short one.”

The “Manual” thus foreshadowed became the voluminous treatise “The Elements of Quaternions.”

Those interested in the future of Quaternions will have welcomed the new edition of this work brought out by the present occupant of Hamilton’s Chair, Prof. Charles Joly, who has himself also added some remarkable developments to one branch of the subject, the Theory of the Linear Vector Equation.

Hamilton’s Quaternions may be viewed in two lights, as a development of the logic and philosophy of symbols in their relation to space of three dimensions and also as an instrument of research in Geometry and Physics. In the former aspect, the Quaternions will ever remain a splendid monument of the imagination and genius of its inventor. In the latter point of view, that is when we come to regard it as a working calculus, it would be premature as yet to fix the place it will ultimately occupy.

A few years after Hamilton had entered upon his scientific career, James MacCullagh won his Fellowship in Trinity College. After an interval of three years he was appointed Professor of Mathematics, and eight years later succeeded Dr. Lloyd in the Chair of Natural Philosophy. It would be difficult to over-estimate the stimulating effect of MacCullagh’s lectures as Professor upon the Mathematical School. Many of those whose names stand out afterwards—such men as Jellett, Michael and William Roberts, Haughton, Townsend and our present honoured Provost—were MacCullagh’s pupils. To the present day the tradition still lingers in Trinity College of the impression MacCullagh made upon the minds of those with whom he came in contact.

When, passing from his influence as a teacher, we come to examine his own original work, we find that this naturally divides itself into two departments, the first embracing Geometry and that part of the field of Mathematical Physics which most resembles Geometry, that in which the fundamental principles are entirely agreed upon; the second his work in Physical Optics, where he has to imagine new principles which, mathematically developed, should correlate the empirical laws hitherto obtained and be capable of verification by experiment.

Of the first class we have his studies in “Surfaces of the Second Degree.” The most striking result he here obtained was the discovery of the modular generation of the quadric, thus extending to surfaces the focus-and-directrix property of the conic in plano. We are also indebted to him for some very elegant theorems in the theory of confocal quadrics, a subject to which he devoted much attention. He likewise gave a course of lectures containing a masterly discussion and geometrical pre-

sented of the motion of a rigid body round a fixed point not acted on by external forces.

At the very outset of his career as an original author he seems to have been attracted by the theory of Light. To understand the ardour with which MacCullagh and his contemporaries devoted their mathematical powers to Physical Optics, we must endeavour to recall the circumstances of the time. The celebrated memoirs of Fresnel had recently appeared. In these he had proved, following Young, that the ethereal vibrations which constitute Light must be in the plane of the wave-front; that a beam of polarised light was simply a system of parallel waves in which these transverse vibrations were all in one direction. He had applied the theory of the ellipsoid to prove that there were three directions in a crystal in which the restitution-force coincided with the direction of the vibrations; that in the plane of every wave there are two directions along which, if a particle vibrates, the component of the restitution-force resolved in the plane of the wave will be along the direction of displacement. He had also from these principles deduced the equation of his famous wave-surface.

How much the work of Fresnel filled the imagination of scientific men in those days may be seen from the enthusiastic language which the sober-minded Dr. Humphrey Lloyd allows himself to use about him in his valuable report on Physical Optics, which he wrote for this Association in 1834.

In passing I would say that the name of Fresnel reminds us of the loss Science, and especially this Section, has sustained since we last met in the death of that illustrious French physicist who devoted his life with such ardour and success to the same field of research—Alfred Cornu. Those of us who had the privilege of being present will recall with a sad pleasure the beautiful address he gave us in Cambridge on the Wave Theory of Light on the occasion of Sir George Stokes’ jubilee.

Fresnel in his analysis had assumed that when the molecules of the ether are disturbed by the passage of a wave the force of restitution acting upon a molecule depends upon that molecule’s absolute displacement. Cauchy and Neumann and, in England, Green, improved on Fresnel’s reasoning, making this force depend, not on the absolute, but on the relative displacement; all these physicists, however, worked on the lines of endeavouring to form an explanation of the propagation of the waves of Light, by treating them as the waves in an elastic medium, akin in its properties to a solid medium in which the stresses depend on the deformation of the elements.

MacCullagh agreed with these others in making the forces of restitution depend on the relative displacements as expressed through a certain function  $V$ , which represented the potential energy of the medium. In the further development of the theory he, however, diverges from them and adopts a line of his own. Struck by the significance of the fact, to which he seems to have been the first to direct attention, that the vector whose components are

$$\frac{1}{2} \left( \frac{dw}{dz} - \frac{dw}{dy} \right), \frac{1}{2} \left( \frac{dw}{dx} - \frac{dw}{dz} \right), \frac{1}{2} \left( \frac{dw}{dy} - \frac{dw}{dx} \right),$$

which we now, of course, know as the vector of molecular rotational displacement, was, so to speak, a physical vector, independent of the choice of our axes of coordinates, he was led to the idea of choosing for the form of  $V$  that of a homogeneous quadric in these three components. It must be admitted that the reasoning by which he attempts to prove the necessity of this assumption is eminently unsatisfactory, and that the assumption itself lay open to an apparently fatal objection urged later by Stokes, that of neglecting to secure the equilibrium of the element of the medium quoad moments.

Having, however, adopted this form of  $V$ , MacCullagh proceeds (making the assumption that while the elasticity of the medium varied the density was everywhere the same), by processes of remarkable elegance and simplicity, to develop the laws of wave propagation in a crystal, thus verifying the wave-surface of Fresnel, while at the same time he found himself able to satisfy completely the requirements at the limits. He could also point to experience, *e.g.*, the experiments of Brewster and Seebeck, as justifying the simple and beautiful laws which he had succeeded in obtaining.

Nevertheless the force of Stokes’ objection was felt to be so strong that one who reviewed the subject, say thirty years ago, would have regarded MacCullagh’s work in Optics as presenting indeed opportunities for beautiful mathematical developments, but lacking sound physical basis.

The publication, however, of the epoch-making treatise of Maxwell on Electricity and Magnetism entirely changed the aspect of the question, and in particular threw a new light on MacCullagh's assumption. FitzGerald, in 1879, pointed out that the Potential Energy, which in Maxwell's theory was equivalent to the electrostatic energy, really was a quadratic function of three variables, which answered to the components of MacCullagh's molecular rotation, and accordingly led to the same differential equations of the motion as MacCullagh had deduced.

Subsequently Larmor, in his remarkable investigation of the Dynamical Theory of the Electric and Luminiferous Ether, deliberately reconsiders MacCullagh's position, finds in fact in his equations the starting point of his own theory. He points out the real significance of MacCullagh's function  $V$ ; that it corresponds to a stress-strain system, but one of a very novel type; one in which the stresses depend entirely on the rotational displacements of the molecules, and are otherwise absolutely unaffected by the ordinary deformation-strains. He further shows that the difficulty under which MacCullagh's theory laboured, that it did not provide for the rotatory equilibrium of the element, could be removed if we allowed ourselves to assume the existence of a hidden torque acting on each element.

As I understand the advocates of this theory, they maintain that an important step has been made, even though in the present state of our knowledge we may not be able to account for the existence of this hidden torque. They point out, however, that such a torque is at least not inconceivable, whether its explanation be sought in concealed kinetic phenomena, as in Lord Kelvin's material gyrostatically constituted medium, or in quasi-magnetic forces supposed to reside in the ethereal elements.

Should this theory of a rotationally elastic ether obtain final acceptance, it will of course be a matter of congratulation to MacCullagh's countrymen to find that his labours, in this, perhaps the most important field of his researches, have not been thrown away; that they represent no mere play of elegant mathematical analysis, but a real step in the progress of physical science.

A few years after MacCullagh, two other well-known men, whose names for half a century were associated with the Mathematical School in Dublin, were elected Fellows—Andrew Searle Hart, afterwards Sir Andrew Hart, and Charles Graves, subsequently Bishop of Limerick. They won their Fellowships in two successive years, and both lived to an advanced age.

Hart had a great reputation as a geometer. His examination papers were specially noted for the number of original problems they contained. As specimens of his work we may instance the following. Extending Feuerbach's theorem for the nine-point circle, Hart showed that the circles which touch three given circles can be distributed into sets of four all touched by the same circle. He also showed that Poncelet's beautiful porism for coaxial circles in plane held for the surface of an ellipsoid, if we replace the rectilinear polygons by geodetic polygons and the coaxial circles by lines of curvature.

Graves became Professor of Mathematics on MacCullagh's resigning the Chair in 1843. He was largely influenced by the writings of Chasles, of whose two memoirs on Cones and Spherical Conics he published a translation. In this were incorporated valuable original additions of his own, amongst others the remarkable theorem that if two spherical ellipses are confocal the sum of the tangents drawn to the inner from any point of the outer exceeds the intercepted arc between the points of contact by a constant length, a theorem which of course includes the corresponding proposition for confocals in plane. Graves was one of the first to apply the method of the Separation of Symbols to Differential Equations, and gave an elegant demonstration by this method of Jacobi's celebrated test for distinguishing between maxima and minima in the Calculus of Variations.

On the death of MacCullagh it was determined to strengthen the Natural Philosophy department by the establishment of a second Professorship in that subject, and Jellett, one of the ablest of MacCullagh's pupils, was appointed to the new Chair.

His first published work was his "Calculus of Variations," which at the time it was written constituted the only systematic English treatise on the subject. It is marked by that peculiar acuteness and power of fastening on essential points, whether for criticism or exposition, which was the author's leading characteristic. Apart from the excellent account he gives of the

researches of Continental mathematicians, I would notice especially his most interesting chapters on the conditions of integrability and many valuable geometrical theorems on surfaces hence resulting. In discussing his more properly original work we may arrange it in three divisions: 1st, his papers on Elasticity; 2nd, that on the properties of Inextensible Surfaces; 3rd, those on the application of polarised light to the new subject of Chemical Equilibrium.

In taking up the problem of an elastic medium and the propagation of waves in such medium, Jellett follows the example of MacCullagh, who had made this subject one of special interest to the Dublin school. In these memoirs he draws attention to a remarkable difference in the mode of regarding the molecular constitution of the medium, a difference corresponding to what is now known as the distinction between the Rari-constant and Multi-constant theories. We may, Jellett points out, regard the action between two molecules as only conditioned by the relative position of these molecules, or as dependent also on the position of the neighbouring molecules. The first is termed by Jellett the hypothesis of independent action, and this he shows to lie at the basis of Cauchy's theory, whereas the theory of Green, the English elastician, essentially involves the second hypothesis which Jellett calls "modified action." He established in the same papers the important theorem that if a Work function exists the three directions of vibration, corresponding to a plane-wave, are rectangular, and *vice versa*.

In his memoir on Inextensible Surfaces various interesting questions are discussed. He proves that in the case of a synclastic surface if a closed curve on the surface be held fixed, the entire surface will be immovable; that on the other hand on an anticlastic surface it is possible to draw a curve which may be held fixed without involving the immovability of the surface, the conditions being that the curve will be that formed by the successive elements of the inflectional tangents. The mathematical theory of such curves had been already studied, but Jellett seems to have been the first to signalise their importance in the theory of deformation, and, on account of the property referred to, he proposed to call them Curves of Flexure. It is interesting to remark that Maxwell was attracted by the same subject of Inextensible Surfaces, and in one of his earliest papers confirms by an entirely different method several of Jellett's conclusions.

At the close of Jellett's paper a remarkable proposition is laid down, apparently for the first time, that a closed oval surface cannot be inextensibly deformed; in other words, that if such a surface be perfectly inextensible it is also perfectly rigid. I think we must admit that the proof of this striking theorem offered by Jellett is by no means satisfactory. Subsequent attempts by others to establish this proposition can hardly be said to be more successful. But the fact that it can be rigorously proved true for a sphere or more generally for any ellipsoid seems to indicate that we have here to do with a real and important theorem, but one which needs, as is so often the case, to have the limits of its application more clearly defined.

Many experimental physicists will know Jellett best by the beautiful and delicate instrument he invented, "The Double-plane Analyser," an instrument which he devised in order to secure the more exact determination of the rotation of the plane of polarisation than could be obtained by the polariscopes hitherto in use. Jellett was actuated here by the consideration that he saw in this phenomenon of the rotation of the plane of polarisation a means of attacking the interesting problem of chemical equilibrium. Chemical equilibrium he defines thus: "Two or more substances may be said to be in chemical equilibrium, if they can be brought into chemical presence of each other (as in a solution) without the formation of any new compound or change in the amount of any of the former compounds which have thus been brought together." In a mixed solution of sundry bases and acids where all the possible salts are soluble, what are the proportions in which the acids are distributed amongst the bases? Such was Jellett's question, and in answering it he arrives by a remarkable train of quasi-mathematical reasoning at certain laws governing this distribution, and proceeds to establish the truth of these laws by observation with his new polariscope.

He also discusses in the same papers two alternative theories which we can hold of chemical combination, the "statical" and the "dynamical," and shows from the consideration of the number of equations which subsist that the "dynamical theory" is alone admissible.



When the Association met in Belfast twenty-eight years ago Dr. Jellett occupied this Chair, and at the close of his Address, in which he took for his subject certain fresh applications of Mathematical Analysis to Physical Science, he touched upon these very researches in which he was at the time engaged.

All old Trinity men would think this enumeration incomplete if it did not refer to the wonderfully active animating presence of Samuel Haughton. He also directed his energies in the first instance to the subject of Elasticity, on which he wrote several important memoirs, endeavouring to formulate a system of laws by which he might be able to explain the propagation of Light. But apparently discouraged by the extreme difficulty of the problem his versatile brain turned soon to quite other branches of science—to Physical Geology, then to Physiology and Medical Science, and in fact in his later work he passes out of the cognisance of Section A.

Of the pure mathematicians trained under MacCullagh two of the most eminent were the twin brothers Michael and William Roberts. Strikingly alike in their personal appearance they were in my student days two of the best known figures in the Courts of Trinity.

In his geometrical work Michael Roberts pursued the fruitful lines of research started by Chasles and followed up by MacCullagh in the study of quadric surfaces, and it fell to his lot to discover some most remarkable theorems on the relations of the geodetics on the surface to the lines of curvature; theorems indeed to which the author would have been justified in applying words which Gauss used of a great theorem of his own:

"Theoremata quæ ni fallimur ad elegantissima referenda esse videntur."

Joachimsthal had shown that the first integral of the equation of the geodetics on an ellipsoid could be thrown into the well-known form  $PD = \text{constant}$ . Michael Roberts now showed that the geodetics, which issue in all directions from an umbilic, pass through the opposite umbilic where they meet again by paths of equal length; that the lines of curvature considered with respect to two interior umbilics possess properties closely analogous to those of the plane conic with respect to its foci; that if such umbilics A and B be joined by geodetics to any point P on a given line of curvature they make equal angles with such line, and consequently that as P moves along the line of curvature, either  $PA + PB$  or  $PA - PB$  remains constant, so that if the ends of a string be fastened at the two umbilics and a style move over the surface of the ellipsoid, keeping the string stretched, the style will describe a line of curvature. Another remarkable analogue he proved was the following: that as in a plane conic if a point P on the curve be joined to the foci A and B,

$$\begin{aligned} \tan \frac{1}{2}(PAB) \tan \frac{1}{2}(PBA) &= \text{const.} \\ \text{or } \tan \frac{1}{2}(PAB)/\tan \frac{1}{2}(PBA) &= \text{const.} \end{aligned}$$

so precisely the same relation holds for a line of curvature on the quadric, replacing the foci by the umbilics and the right lines by geodetics.

Sir Andrew Hart made a valuable contribution to the subject by investigating the relation between the angles which an umbilical geodetic makes with the principal plane when it leaves the umbilic and when it returns to it again after going the circuit of the surface. He proved that if  $\omega$  and  $\omega'$  be these angles,  $\tan \frac{1}{2}\omega'$  can be expressed by means of complete elliptic integrals independent of  $\omega$ . This is interesting, as it shows that such a geodetic is not a finite closed curve, but that it crosses itself over and over again at the umbilics, the successive values of  $\tan \frac{1}{2}\omega$  forming a geometric series.

To Michael Roberts is also due much important work in the department of pure analysis—notably, in modern Algebra his method of deriving Covariants, and the investigation of their relations by means of their sources, and in the theory of Abelian integrals his construction (following the method of Jacobi) of a Trigonometry of the hyperelliptic functions.

His brother William Roberts is perhaps best known for some of the investigations he carried out by means of elliptic co-ordinates. For example, he applied them to Fresnel's wave-surface, and showed that the two sheets of the surface can be expressed in the simple forms

$$\lambda^2 + \nu^2 = a^2 + b^2 - c^2 \quad \text{and} \quad \lambda^2 + \mu^2 = a^2 + b^2 - c^2.$$

By following the same method he succeeded also in adding an interesting new triple system of orthogonal surfaces to those already known.

Richard Townsend was another of the Fellows of Trinity of MacCullagh's school. He was known to us in College in my day as the great expositor of the new geometry of Anharmonics and Involution. He wrote many valuable original papers, but it was as a lecturer he was most remarkable. I never met a teacher so enthusiastic or one who seemed to enjoy teaching more thoroughly.

He inspired his pupils with much of his own ardour, and it is greatly owing to Townsend's influence that the old name Trinity had for the study of Geometry was so well kept up in his day.

He published in the latter part of his life an extensive treatise on Modern Geometry, which did good service in presenting the subject in the light of an organised system and not as a collection of isolated problems.

In this connection I must not omit to mention one of our most original Irish geometers of recent days, Dr. John Casey. Where Casey learnt his Mathematics is indeed a marvel. Up to middle life he was engaged in the engrossing labour of a schoolmaster in Kilkenny under the National Board of Education. It was not till he was nearly forty that by the advice of Townsend, to whom he used to send up some of his ingenious geometrical solutions, he moved up to Dublin and entered Trinity College. Of his original papers his best known are those on Bicircular Quartics and Cyclides.

In elementary Geometry we owe to him a very elegant extension of Ptolemy's famous theorem that for four points, ABCD, on a circle  $AC \cdot BD = AB \cdot CD + AD \cdot BC$ . Casey shows that the same equation is true if we replace the four points by four circles touching a common circle and the lines joining the points by the common tangents to the circles. He acquired so high a repute both as a teacher and as a writer that he was offered and accepted the post of Professor of Mathematics in the Catholic University.

It is not yet two years since George FitzGerald was taken from us. The many loving tributes to his memory which appeared in the scientific journals after his death reveal to us how deep and widespread his loss was felt to be, but it is in Ireland this loss is most serious. As long as he lived and worked, our country could claim to own one of the foremost members of that select band who are endeavouring to wrest from Nature her inmost secrets.

You know how sedulous an attendant he was of the meetings of this Section, and Trinity College never sent you a representative of whom she had more reason to be proud, for he has done more than any of her sons for many years to maintain the reputation of her scientific school. This he has brought about, not by his writings only, able and original as these were, but also by the encouragement and stimulus he gave the younger men he gathered round him, and the self-forgetful readiness with which he gave all the help he could to those who in any measure shared his own genuine love for science.

You will all rejoice that we are now in possession of a volume containing a complete collection of FitzGerald's scientific papers. I am sure he himself could not have wished for a better chronicler of his life and labour than his intimate friend Dr. Larmor, more especially as Dr. Larmor's own far-reaching speculations on the great mystery of the ether qualify him in a very peculiar manner to appreciate the work of his fellow-physicist. The admirable analysis of that work in the opening pages of this volume renders any further account of it on my part completely unnecessary.

A few months before FitzGerald's death there passed away one of his most distinguished pupils, Thomas Preston. Though cut off so young he had already done much work, and of a quality which raised high expectations of his future. His treatises on Light and on Heat are to be noted, not merely for the excellent account they give of the recent additions to the subjects treated, but for the thoughtful and philosophic spirit in which the whole is presented. It was, however, his experimental researches which most excited attention, more particularly those on the action on Light of a strong electro-magnetic field and the fine experiments in which he extended beyond any observations hitherto made the analysis of the Zeeman effect.

Of two others I have yet to speak, and these were emphatically representatives of this city and of the College in whose Halls we are meeting to-day—Thomas Andrews and James Thomson. It would be difficult to describe adequately all the phases of so manifold an activity as that of Dr. Andrews. As one long associated with him as a colleague I would bear testimony to one side of his life-work—the potent influence he

exercised in this College in its earlier years as a skilful pilot guiding the ship till it was well out of port. His high ideal of the function it should discharge in the education of the country and the practical zeal and ability which he ever brought to bear on the administration of our affairs contributed in no small measure to place the College in the assured position it occupies to-day.

On his great physical and chemical investigations it is happily the less necessary for me to touch, as they have been so fully brought before you by our President in his opening Address; and as regards the most important of these researches, those on the continuity of the Liquid and Gaseous states, no one assuredly could have more fitly expounded them than one who has himself pressed forward with such splendid success in the paths which Andrews opened up.

I have always considered that Andrews, through the long course of these later researches, was most fortunate in having near at hand such a friend as James Thomson; not that he was a collaborator—for Andrews did all this work unaided—but that Thomson gave him throughout that best of all encouragement which consists in enlightened appreciation of the importance of the results he was obtaining and of their inner meaning and significance.

Of Thomson himself what shall I say? Of all the scientific men I have come across he perhaps most fulfilled the idea of a philosopher, his ever-working brain ever seeking out causes, ever pondering on the why and the wherefore of the unexplained.

One of his earliest investigations is perhaps the best known, that in which, basing his reasoning on Carnot's principle, he demonstrates the effect of pressure in lowering the freezing-point of water, and in which he gave at the same time a numerical estimate to this effect.

This discovery was of great practical import, for, small as the effect was, it enabled him to explain fully the rationale of the plasticity of ice.

Forbes had already shown that the motion of glaciers depended upon a plastic or viscous quality in the ice. It remained for Thomson, by the aid of his newly discovered principle, to go a step further and account for this plasticity.

It is interesting to note that the questions which led to some of his most valuable investigations seem to have been started by the filial task he took upon himself of re-editing his father's educational text-books. It was, for example, the revision of a chapter in his father's Geography which I believe led him to examine more thoroughly into Hadley's theory of the Trade winds, and to make the following important addition to that theory. He showed that while in the tropical latitudes, say of our northern hemisphere, two currents would satisfy all the conditions, *i.e.*, the Trade wind blowing from N.E. to S.W. in the lower regions of the atmosphere, and the return current in the upper regions, on the other hand that in the temperate latitudes there must be three currents at different elevations; that the uppermost and the lowest of these have a movement towards the Pole, but in the middle regions of the atmosphere between these there must be a large return current from the Pole, and that the prevailing motions of all three currents would be from west to east.

Thomson was particularly successful in his treatment of this and other questions of fluid motion. He was not familiar with the technique of the higher mathematics, and on this very account was not tempted, as so many mathematical experts are, to assume impossible conditions in order to bring the problems within reach of their algebraic analysis; but for all that his mind was eminently of a mathematical cast. He is never vague or loose in his reasoning, and he had a wonderfully tenacious grasp of physical principles. The result was that he has succeeded in finding out the key to some of the most curious phenomena in the motions of fluids.

I may give as a typical instance of his line of reasoning his beautiful explanation of the action of the water of a river flowing round a bend. He saw clearly that from true dynamical principles the flow of the water must be most rapid near the inner bank, and the question which presented itself to his mind was why then the inner bank was not worn away. The answer he showed to consist in the friction of the bed checking the velocity of the lowest stratum of the water. The effect of this he proves to be that an under-current is produced in this stratum across the bed of the river from the outer towards the inner bank, a current which does two things: it carries sand and

detritus and deposits them on the inner bank; and, since the water in this current has to rise vertically to the surface when it reaches this bank, it thus protects it from the scour.

In a review of Thomson's work we should emphasise his constant endeavour, whether in Mathematics or Physics, to attain clear conceptions of fundamental principles. This showed itself in the various innovations in nomenclature he introduced. Many of the new words he coined, "radian," "numeric," "torque," "interface," "clinure," "posure," &c., are great helps both in thinking and teaching.

The same determination at any cost of hard thinking to arrive at clearness in regard to fundamental principles is strikingly evidenced by one of his later papers, that on the "Law of Inertia and the Principle of Chronometry," which is a most searching discussion of the true significance of Newton's first and second laws of motion.

I must now close this review. I shall be glad if I have succeeded, however imperfectly, in giving you some impression of our Irish schools of Mathematics and Physics, of the workers and of the sources from which they drew their inspiration. There surely never was a time when the problems presented to the mathematician by Physical Science were more interesting; never a time when Science for its onward progress stood more in need of those gifted ones who combine clearness of thought with imagination and hopeful courage. Let us hope that amongst these in this new century, others of our countrymen may be found not unworthy to have their names inscribed in the roll which contains those of Hamilton and MacCullagh, of Andrews and Thomson.

#### NOTES.

WE record, with very deep regret, the death of Prof. Virchow, on September 5, in his eighty-first year. The State funeral accorded to Prof. Virchow took place in Berlin on Tuesday. Among those present were the Prussian Ministers of Education and Finance, the Foreign Secretary, the Chief Burgomaster of Berlin, and numerous representatives of Berlin and other universities, and of learned and scientific societies—both German and foreign. After the funeral service, orations were delivered, in which Prof. Virchow was considered as man of science, politician and municipal reformer. At the meeting of the Paris Academy of Sciences on Monday, a eulogy on Prof. Virchow was delivered by M. Bouchard.

WE have also to announce the death of Sir Frederic Abel, on September 6, in his seventy-sixth year.

THE next meeting of the Australasian Association is to be held in Dunedin, New Zealand, in January, 1904, and the following have been appointed presidents of sections:—B—Chemistry: J. Brownlie Henderson, Brisbane. C—Geology and mineralogy: W. H. Twelvetees, Hobart. D—Biology: Colonel W. V. Legge, R.A., Hobart. E—Geography: Prof. J. W. Gregory, Melbourne. F—Anthropology and philology: A. W. Howitt, Melbourne. G—Economics, subsection 2, agriculture: J. D. Towar, Roseworthy, South Australia. H—Architecture, engineering and mining: H. Deane, Sydney. I—Sanitary science and hygiene: Dr. Frank Tidswell, Sydney. J—Mental science and education: John Shirley, Brisbane.

THE annual congress of the Sanitary Institute was opened at Manchester on Tuesday last, when some two thousand delegates were present from all parts of the country.

IN connection with the celebration of the 100th anniversary of the birth of Niels Henrik Abel, now in progress at Christiania, twenty-nine foreign men of science on Saturday last received the degree of Doctor, *Honoris Causa*; among the number were Lord Kelvin, Lord Rayleigh, Sir George Stokes, Prof. G. H. Darwin, Prof. Forsyth, and the Rev. George Salmon, Provost of Trinity College, Dublin.

THE Punjab Government has, according to the special Indian correspondents of the *Lancet* and the *British Medical Journal*,